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NOTES, CRITICAL AND EXPLANATORY, ON THE MAGICAL PAPYRI.

Now, when, as it seems, the remnants of ancient superstition are, at last, about to be critically edited, I think it the right moment to contribute to their study a number of notes, which,—now for a considerable time,—have accumulated among my papers. The texts which I use are: (1) Parthey, *Zwei griech. Zauberpapyri*, Berl. Akad. Abh. 1865 (B. 1, B. 2); (2) Dieterich, *Jahrb. f. Philol.* Suppl. xvi. (V); Abraxas, Leipzig 1891 (W); (3) Wessely, *Griech. Zauberpapyrus*, Wien. Denkschr. xxxvi.: *Neue griech. Zauberpapyrus*, *ibid.* xlii. (We. i. We. ii.).

V, i. 33. A certain number of birds shall be strangled *μέχρις οὗ ἕκαστον τῶν ζώων ἀποπνιγῇ* <καὶ τὸ αἶμα αὐτῶν εἰς αὐτὸν ἐλθῇ>; thus Leemans and Dieterich, whose reference to We. i. Par 40 is useless, as there the cock is to be butchered. We must read: <καὶ τὸ πνεῦμα. For the sacrifice is offered to a wax doll representing Eros. To this the breath of the victims shall give life and breath. We must picture to ourselves the animals as strangled right before the face of the image (cp. 32, 33: ἀναπνίξεις αἶμα προσφέρων τῷ Ἐρωτι), so that their breath reaches it. Thus the last breath of a dying man was taken up by his next of kin with their mouths in order to continue the existence of his spirit; Tyler, *Primitive Culture*, i. 433; E. Rohde, *Psyche* 22, 1.

V, iii. 27 ff. might well be added to Mr. H. C. Trumbull's long list of threshold-sacrifices in his new book (*The Threshold-*

Covenant). But what is ὠὸν ὄρνιθος ἀρσενικοῦ? Certainly, no monstrous curiosity, but simply an egg which would hatch a male chicken (ὄρνις here used for fowl, as so often). It was a current belief among the ancients that long, pointed eggs contained male birds (Columella, viii. 5, 11; cp. Aristotle, π. ζω. γεν. iii. 27).

V, 4, 3. ὄνειρον π...αι Pap. πέμψαι Leemans, πέμπε Dieterich. But πέμψαι is sufficient; for the meaning of the passage is: write on a tablet the following charm and the dream which you want to send and put this into the mouth of a cat.

V, v. 1 read καὶ τὰ κρέα σου instead of κράτέα: he will give thy flesh to the dogs.

V, 5, 11, 12. χρημάτων, εἰ περὶ τοῦδε, περὶ πάντων (!) πυνθάνω. Knoll (*Philol.* liv. 560), wants to read εἰ<πὲ> περὶ τοῦδε a.s.f. But it is better to add another εἰ before περὶ πάντων.

V, 8, 6 f. πρὸς ἀρμονίαν τῶν ἐπτὰ φθόγων ἔχόντων φωνὰς πρὸς τὰ κη φῶτα τῆς σελήνης, and identically recurring W. xvii. 30 (Abraxas 196, 2). Dieterich apparently has no explanation to offer. Yet, as far as I know, these two quotations give the earliest, if not the only mention in a Greek author of the 'stations of the moon' the nakshatras, which play so prominent a part in Indian astrology.

V, 9, 4. Before θεὲ μέγιστε a lacuna must be assumed to exist, as otherwise the charmsong would begin too abruptly. The words θεὲ μέγιστε ὅς ὑπερβάλλεις τὴν πάντων δύναμιν have been used to fill this blank.

Par. 286 ff. (We. 51). This incantation, which would have deserved a place in Heim's *Incantamenta magica*, seems to have been metrical. Thus we read 287 the rest of a senarius: *χειρὶ πενταδακτύλῳ* and a complete verse, slightly corrupted still exists 290: *οὐκέτι βρεχέσεται ποτ' ἐν βίῳ πάλιν* (πωποτε Pap.).

Par. 296 ff. (We. i. 52). It is interesting to notice that a similar group to the one described here was used by the Egyptians as a seal to brand sacrificial animals; see Plut. *Is. et. Osir.* 31.

Par. 475 ff. (We. i. 56) read: *Ἰλαθί μοι Πρόνοια καὶ Τύχη τάδε γράφοντι* (γραφεντι Pap.) *τὰ πατροπαράδοτα* (πρατὰ παράδοτα We.) *μυστήρια*.

Par. 484 (We. i. 57). *Μίθρας ἐκέλευσέν μοι μεταδοθῆναι ὑπὸ τοῦ ἀρχαγγέλου αὐτοῦ, ὅπως ἐγὼ μόνος ΑΙΗΤΗΣ οὐρανὸν βαίνω*. In these letters either *μύστης* or *μνητής* is hidden. *Μύστης*, at the first glance, would seem more probable, on account of the *μόνος*. But 477 *μόνῳ δὲ τέκνῳ ἀξίῳ μνείσθαι* (Dieterich Abr. 163, 4) and 732-3 *ἐὰν δὲ θέλης καὶ σὺν μύστῃ χρῆσασθαι* make *μνητής* preferable. I do not think it necessary to insert *εἰς* before *οὐρανόν*.

Par. 530 (We. i. 58) read: *ἔστιν μοι θνητὸν γεγῶτα συνα<ν>ιέναι ταῖς χρυσοειδέσιν μαρμαρυγαῖς*. For the magician himself becomes a star 574: *εἰμὶ σύμπαντος ὕμιν ἀστῆρ*.

Par. 633 ff. (We. i. 60) read *στραφήσονται ἐπὶ σε αἱ ἀκτῖνες· ἔσιδε <δὲ> (εσειδε Pap.) θείων μέσον· ὅταν οὖν τοῦτο ποιήσης, ὄψει θεόν*.

The verses 662 to 705 have been well explained by Dieterich (Abr. 105) as referring to the god Mithras. But I must object to his referring the *μόσχον ὦμον χρύσεον*, ὅς ἐστιν ἄρκτος ἢ κινούσα οὐρανόν 699 ff. to the same group of ideas. For the hindleg of the ox is from remotest antiquity the Egyptian constellation of what we call the Wain: cp. Lepsius' *Chronologie der alten Aegypten*.

Par. 745 (We. i. 63): for *αὐτονῶ φθόγγῳ* read *ἀτόνῳ* 'in a low voice'.

The verses 835 ff. (We. i. 65) give an astrological piece, which here is entirely out of place. It is, however, very important, as it proves that these magical papyri are only somebody's inconsiderate attempt to gather a number of stray charms into a larger collection (cp. Dieterich, *Jahrb. Suppl.* xvi. 758). In this way our little fragment, apparently once a part of an elaborate horoscope, came to be embodied in the Parisian papyrus, albeit it possesses

no magical meaning whatever. But Wessely was utterly wrong in affixing to it the title 'Stufenjahre' or climacteric years. A comparison with Vettius Valens, an astrologer of the second century A.D., soon to be edited, shows the real meaning of the fragment. The sixth chapter in the sixth book of his *ἀνθολογαί* treats *περὶ τῆς εἰς δέκα ἔτη μῆνας ἐννέα διαιρέσεως ἐμπράκτων τε καὶ ἀπράκτων χρόνων*. That is, a distribution of the life among the planets according to a fixed interval of ten years and nine months, during which period the ruling planet was the *ἀφότης* or principal factor in determining its events. About this Saumaise wrote at some length in his 'anni climacterici.' Wessely's misnomer is due to a superficial perusal of this book.

Par. 1065 ff. (We. i. 71): *τῆς αὐγῆς ἀπόλυσις· χωχω Ω χωχω* (it is an anagram) *ιερά αὐγὴ ἵνα καὶ ἡ αὐγὴ ἀπέλθῃ· χώρει ἱερά αὐγὴ, χώρει καλὸν καὶ ἱερὸν φῶς τοῦ ὑψίστου θεοῦ*. The proper order of these words was: *τῆς αὐγῆς ἀπόλυσις, ἵνα καὶ ἡ ἱερά αὐγὴ ἀπέλθῃ* (καὶ because the *ἀπόλυσις* of the god himself had been given 1035 ff.) *χι-χι· χώρει* and so forth. The words *ιερά αὐγὴ* after the Ephesium gramma must be struck out, as wrongly repeated.

Par. 1079 (*ibid*) we have in *ἀνειδωλόπληκτον* an interesting proof of the tenacity with which superstitious beliefs again and again creep forth. For hitting 'πλήγειν' is the most dreaded action of sprites: cp. Aristoph. *Birds* 1492. From this very inclination the *ἥρωες* = souls had, at a later time, even been named *πλήκται*: Rohde, *Psyche* 225, 4. Cp. also Brit. Mus. 120, 240 (We. ii. 27).

In the hymn to the Moon (2242 ff.), which has been partly restored by Wessely (We. i. 31) one complete senarius can be added after his verse 25: *ὀλκῆτι, λοφαίη, φασγάνων θυμάντρια* (2267). Among the *disiecta membra* from here to 2285, where Wessely's restoration again begins, a number of Greek words can be found by slight emendations. 2270 for *σκοπη* read *σκοταίη* or *σκοτεΐη*; 2271 for *νομεή* read *νομαΐη*; 2273 *ινδαλίμη* is adjective formed from *ινδάλλομαι*; *ibid.* *διχθιρα* apparently is *δέχθειρα* = *δέκτειρα* cp. *δέκτρια* from *δεκτήρ*: Archilochos 19 Bgk.; 2275 *μητρι* is *μητρεΐη*. 2276 *εἰδα* is *Ἰδαία*; *ibid.* *λυκω στηλητη* probably *Λυκοπολιτι*. Another complete senarius occurs 2279: *ἀκτῖνας* (belonging to <κε> *καλλισμένην*?) ἢ *σώτειρα Παγγαίη* (I) *Κυτά*. The following line, also, may perhaps be restored thus: *Κλωθαίη, πανδώτειρα* (Hymn. Orph. 10, 16; 26, 2 Abel), *δολιχῇ, κύδιμη*.

Par. 2604 (We. i. 109) *συριστὶ ἡταρον*

κονβυθον πνουσαν. These Ephesia grammata are good Greek: ὄγκον βυθοῦ πνέουσα (ν) and together with τύχη θεῶν καὶ δαιμόνων (2602) they form one complete verse of the διαβολή, which has only been obscured by the inserted abracadabra.

One verse and part of another can be added to the Hecate hymn 2714 ff. (We. i. 114). 2775 we have to read Ἰὼ πασικράτεια καὶ Ἰὼ πᾶσι μεδέουσα, Ἰὼ παντροφέουσα. For Ἰὼ as a name of Hecate or rather Selene cp. Malalas in Lobeck's *Aglaophamus* 401/2 note†: οἱ Ἀργεῖοι μυστικῶς τὸ ὄνομα τῆς σελήνης τὸ ἀπόκρυφον Ἰὼ λέγουσιν ἕως ἄρτι; and to παντροφέουσα see the numerous beliefs about growth and decreescence of all things in sympathy with the waning or filling moon: Roscher, *Selene*, 64 ff. 184 ff.; Pauly-Wissowa i. 39.

Par. 3096 (We. i. 122). Among the ingredients of a sacrifice occurs σιλούρου καρδία. Wessely as well as Dieterich (Abraxas 79) change this into αἰλούρου. However, a sacred fish σιλουρος was found in the Nile (Wiedemann, notes on Herodotus ii. p. 176). It was believed to be connected with the dog-star and with thunder storms; cp. Pliny *H.N.* 9.58: fluviatili silurus caniculae exortu sideratur et alias semper fulgure sopitur.

Par. 3119 ff. (We. i. 123). It is well known that great power was attributed to a certain order of words and letters. In this connection it is interesting to notice that the ἀπόλυσις contained in these verses is formed by exactly the same letters, but in inverted order, by means of which 3103 ff. the god had been conjured.

Par. 3173 (We. i. 124). 'Certain reeds must be cut πρὸ ἡλίου ἀνατολῆς, μετὰ δυσμᾶς . . of the Sun himself? We must add σελήνης; this word was all the more likely to drop out after a C as it almost always in these papyri is only indicated by the sign (C).

Brit. Mus. 46 (We. i. 132 ff.). This papyrus throws an interesting light on the tradition of the sorcerers' handbooks. With verse 176 a 'rhyme' begins abruptly, in which Hermes is implored to reveal a thief. After this, in 185, there comes another spell, to be said over bread and cheese; these, as we hear in 300, are to be kneaded together, and to be given to the people suspected of the theft. But the confusion is not yet at an end. In 200 the ποιήσεις, i.e. the preparations accompanying the magical action, begins, only to be interrupted, however, in 206 after the words ἐπίθες <ἐς> βωμὸν γῆινον, in the very middle of the sentence. Here, in 206, begins the

preparation of a Ἑρμοῦ δακτύλιος, in no way connected with the previous charm. This is brought to an end in 296, and now our manuscript goes on, as if absolutely nothing had intervened, with . . νον (i.e. γῆινον ἐπίθις ζυμῖναν κ.τ.λ. That is to say: the verses 297 and following are the direct continuation of the charm 176-205. This strange confusion is difficult to explain, unless we assume that the compiler of our manuscript left out three columns of his archetype, consisting of thirty lines each, but found out his mistake after he had copied another three columns, and then simply copied the forgotten part, without giving the slightest warning of his mistake to the reader. How very improbable such an explanation is, is apparent. To me it seems that the confusion is older by at least one generation, if not by more. The confused order within the charm itself points to a more rational solution, viz., that the original was compiled from loose scraps, without much regard to their connection, and that the Ἑρμοῦ δακτύλιος found its way into the very centre of the 'theft-charm' simply because in this, too, Hermes was invoked.

B. M. 46, 469/70 (We. i. 138). The demon Ἰδαῖος δάκτυλος, whom Wessely has thought to find here, must give way to the less interesting, but more intelligible words ἴδιον δάκτυλον. The passage will thus read: εἶρας αὐτὸν (the previously described jasper ring) εἰς τὸν ἴδιον τῆς ἀριστερᾶς σου χειρὸς δάκτυλον ἕσω βλέποντα (that is, facing the palm). The ring-finger is the ἴδιος δάκτυλος for wearing a ring. A number of reasons for this relation, from the 'Aegyptiorum sapientia' are given by Macrobius vi. 13, 8 ff.

Pap. Mimant 2391 (We. i. 147), vs. 258 read: δεῦρό μοι, κύριε, ὁ <τὸ φ> ὡς ἀνάγων (ω<πρ>ωσανα<γ>ων We.).

B. M. 121, 309^a ff. (We. ii. 39) read: ἐξαίρων τὸν κύκλον τοῦ ἡλίου καὶ τὴν σελήνην ἐμπεριλαμβάνων (ἐμπερεια . . νον We.); cp. 375 τὴν σελήνην ἐμπεριλαμβάνων.

Ibid. 332 (We. ii. 31) τὸν ἐπὶ τῆς ἰδρυμένον; read γῆς.

Ibid. 388 (We. ii. 33), in a charm, destined to work insomnia <κε . . > νείτω, read <ἀγρ ν π> νείτω.

The Ephesia grammata 393 ff. contain a number of good Greek epithets of Aphrodite, who is here invoked (396 ἐξορκίζω ὑμᾶς, ἅγια ὀνόματα τῆς Κυπρίδος). 393 καμωπί: in this probably Κανωπί<τ ι> is hidden. We might think of Καμήφι, Stob. *Floril.* i. 41, s. 44, p. 288 Mein. But he was a male being. About Kanopus and its orgies see Wiede-

mann on Herod. ii. p. 90 f. 394 *ραδοχ*: probably *ροδόχειρ*; ibidem *ἐρατεν* read *ἐρατεινή*. 395 *εἰσω* probably *Ἰσίη* cp. 500, then *Βουβάστι, ποθῶπι*, and in the same line *φαφιετι* perhaps *Παφίη τε*.

B. M. 121, 679 (We. ii. 43) *ἐὰν μὲν ὁ πρῶτος λύχνος τεταρῇ γνῶσιτι εἰλημπται*. Read *πτάρη*. Compare on this omen Anthol. Pal. vi. 333: *ἤδη, φίλε λύχνε, τρίς ἔπαρες*; Ovid, *Heroid.* 19, 251: *sternuit et lumen*.

Plenty of good Greek words again are hidden in the Ephesia grammata B. M. 121, 948 ff. (We. ii. 51), a love charm, by the help of Aphrodite-Selene. 950 *εἰλαρωτι* read *ἰλαρῶπι*. *ηροδία*: ἡ *Ῥοδία*? 954: *βαυβωφοβειος*: *Βαυβὼ φόβειος* (or *φοβερός*?).

B. M. 121, 986 (We. ii. 52). *ὑπόκειται τὸ ζῳδιον* 987 *ν* (stands probably for *ν=ιον*, the well-known later form of *-ιον* terminations. Repetitions of the last part of words at the beginning of the next line are frequent in the papyri) *θαυμαστὸν τοῦ ζῳδίου ἐν τοῖς πεδίοις*. The sign no doubt is meant as *Ἑρμοῦ* and is the oldest example known to me of the modern and mediaeval symbol of the planet, the herald staff.

B. M. 122, 56, 57 (We. ii. 56): *ἐπίγραφε τὸν τοῦ Ἑρμοῦ*. *τόν* no doubt is a mistake for *τὸ ὄν<ομα>*.

B. M. 122, 48, 49 (We. ii. 56): *τὸ δὲ δευτερό<ν> ὄνομα ἔχον ἀριθμὸν εἰς (5 MS.) τῶν κυριευόντων τοῦ κόσμου τῇ<ν> ψήφον ἔχοντα τῆς πρὸς τὰς ἡμέρας τοῦ ἐνιαυτοῦ*. *εἰς* seems to be certain; the lords of the world are the five planets. On the other hand, it seems not improbable that after *τοῦ κόσμου* a line is missing, although this assumption is by no means necessary. The fifteen letters answering to the *τεῖς ἡμέραι τῆς ἀνατολῆς τῆς σελήνης* seem to me to refer to the number of days during which the moon is waxing, roughly speaking fifteen. But the expression *ἀνατολή* for this period certainly is very singular. A similar use of *ἀνατολή*, however, occurs in B 1, 235-6: *πῆς αὐτὸ ἐπὶ ἡμέρα ζῆ νύστης, ἐξ ἀνατολῆς οὐσῆς τῆς σελήνης*, and B 2, 80: *χρῶ ἐν τοῖς ἀνατολικαῖς*.

Pap. R(ainer) 1, 34 ff. (We. ii. 66, 67). *ὀρκίζω ὅσα ἔστιν πνεύματα ἢ κλαίοντα ἢ γελῶντα φοβερά ἢ ποιοῦντα τὸν ἄνθρωπον δυσόνειρον ἢ ἐκθαμβον ἢ ἀμαυρίαν ποιοῦντα ἢ ἀλλοιωσύνην φρενῶν ἢ ὑποκλοπὴν καὶ ἐν ὕπνῳ καὶ δόξα ὕπνου*. As a whole, Wessely's emendations of these lines are correct. It is only in the *γελῶντα*, where misapplied

knowledge has led him astray. He proposes to read *Γελῶντα* and thinks these sprites are beings after the fashion of Gello, Empusa, and Mormo. Of course, everybody sees that the contrast *κλαίοντα πνεύματα* imperiously demands *γελῶντα*. But I want to call the attention of the reader to the fact, that these lines give us a remarkable insight into the tenacity and vividness of popular belief. For every one of the features here ascribed to the *πνεύματα* exists in modern folklore as well. The whole circle of ideas, in which we find ourselves, has, for the field of Teutonic folk-lore, been treated by Laistner in his admirable *Rätsel der Sphinx*, while on the basis of a 'Hellenistic' relief O. Crusius (the *Philologus* L 102 ff.) has traced part of these ideas through Greek religion. The *πνεύματα κλαίοντα* remind one of the numerous German legends about souls which cry and whimper for salvation (e.g. Grimm, *Sagen* no. 224); the *πνεύματα γελῶντα φοβερά*—this word I take to be adverbial accusative—of the 'höhnisches Lachen' of the 'Kobold' (Grimm, *ibid.* no. 46, 72, 74, 76, 122), and probably also of the strange sounds which caused 'Panic terror.' They make man *δυσόνειρον*, i.e. *ὄνειρώττοντα*: see Crusius cited above; or *ἐκθαμβον*, attonitum; this needs no examples. Or they cause blindness; thus Epizelos was blinded in the battle of Marathon, because he had seen a spectre, Herod. vi. 117; (E. Rohde, *Psyche* 171, 1). They cause furthermore mania; this, too, is too well-known to need any illustrations, except a reference to the booklet *περὶ ἱερῆς νόσου*. But what is meant by (*ποιοῦντα*) *ὑποκλοπὴν καὶ ἐν ὕπνῳ καὶ δόξα ὕπνου*? The word *ὑποκλοπή* is not found in Stephanus, but surely means 'stealthy theft.' Of what? One might think of the 'succubus et incubus' tales. This, however, would have been expressed, if I am right, by *δυσόνειρος*. May we not think of the theft of babies and the substitution of 'change-lings.' The belief exists in modern Greek superstition, where these unhappy beings are called 'children of the Nereids' (Schmidt, *Volksleben* 118). That the fairies like to surprise recently confined women during their sleep is general belief. But they appear, also, under many disguises at other times. For this subtle and unexpected change the expression *ὑποκλοπή* seems to be very happily coined.

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ARISTIDES AT SALAMIS.

§ 1. BETWEEN the invasion of Greece by Datis and Artaphernes and the invasion by Xerxes an important change had taken place at Athens in the military organization. At Marathon the supreme command was still vested in the Polemarch; but in the year 487-6 B.C. the method of lot was introduced for appointing the nine archons, and this innovation necessarily involved the displacement of the Polemarch from the chief command, as that post could obviously not be safely vested in a man chosen by the chances of the lottery. The control of the army was transferred, not to a new Commander-in-chief, but to the body of the Ten *Stratēgoi*, who had hitherto been merely the commanders of the contingents of the ten Cleisthenic tribes. It has been thought that the first idea was that the chief command should rotate among the ten generals, each enjoying it for a day, and that a recollection of this temporary and eminently unpractical arrangement has survived in the well-known anachronistic representation which Herodotus gives of the state of things existing at Marathon. But if such an arrangement was ever actually adopted—for instance in the Aeginetan war¹—it had been luckily condemned and abolished before the great crisis of 480. In that year we find the supreme command entrusted to one man, who is thus in the position of *ἡγεμῶν στρατηγός*. In the earlier part of the civil year 480-79, throughout the campaign of Artemisium and Salamis, Themistocles holds this position; in the later part of the civil year—from the spring of 479 forward—Themistocles has given way to Xanthippus. In the land-campaign of Plataea and in the sea-campaign of Mycale we find Aristides general of the hoplites and Xanthippus general of the triremes.² Thus when the land forces and the sea forces were operating independently, as in B.C. 479, there were two supreme commanders; but where the land forces were acting in subordination to the fleet, as in B.C. 480, there was one supreme commander. This was the arrangement dictated by common sense.

§ 2. We learn from the *Ἀθηναίων Πολιτεία*

¹ It is ingeniously conjectured by Mr. Macan (*Herodotus* 2, p. 145, n. 9) that the circumstance that the Athenian fleet arrived *one day* too late on the occasion of the conspiracy of Nicodromus may have been due to the existence of this absurd system in 487 B.C.

² See Herodotus vii. 197; viii. 131; ix. 28 and 114.

that there was an *ἐπιχειροτομία* of the *Stratēgoi* κατὰ τὴν πρωτανείαν ἐκάστην εἰ δοκοῦσιν καλῶς ἄρχειν· κἂν τινα ἀποχειροτονήσωσιν κρίνουσιν ἐν τῷ δικαστηρίῳ, κἂν μὲν ἄλλῳ τιμῶσιν ὅ τι χρὴ παθεῖν ἢ ἀποτεῖσαι, ἂν δ' ἀποφύγῃ πάλιν ἄρχει (c. 61). If this practice already prevailed in 480 B.C., the question arises whether Themistocles, after his splendid services at Salamis, had to submit to the indignity of such a deposition. In such a matter the expression of Diodorus (or Ephorus) carries no weight, and the statement that *δεξαμένον τοῦ Θεμιστοκλέους τὰς δωρεὰς ὁ δῆμος τῶν Ἀθηναίων ἀπέστησεν στρατηγίας* (Diod. xi. 27) is vitiated by the cause assigned for the act of the people. But it should in any case be pointed out that it is not necessary to assume a formal deposition. The change in the supreme command of the fleet can be fully explained by a difference in views between Themistocles and the other leaders of the confederate army. It is recorded that Themistocles advocated operations in the Hellespont (Herodotus viii. 109), and those are doubtless right who (like Busolt, *G. G.* ii². 717) connect his surrender of the command (why not a formally voluntary surrender?) with his peculiar views as to the general conduct of the campaign.

§ 3. In any case the supreme command in the warfare of 479 B.C. was vested in the two ostracized statesmen Xanthippus and Aristides. When the Persian danger threatened, a decree of amnesty was passed³ permitting ostracized persons, as well as other exiles (with certain exceptions), to return to their country; and the motive of this measure must have been (as Plutarch suggests) the fear that powerful citizens in banishment might medize and do serious hurt to Athens. One expects that Xanthippus and Aristides would have returned as soon as they could, if they intended to return at all. That Xanthippus returned some weeks at least before the battle of Salamis was fought is assumed by the anecdote which Plutarch tells about his dog (*Themist.* 10). But the return of Aristides is described by Herodotus as having occurred in very sensational circumstances on the eve of the battle of Salamis. The syndedion of the Greek generals was sitting; the debate 'either continued all night or was adjourned to an

³ *Ἀθ. πολ.* 23. Compare Stahl, *Rhein. Museum*, 46, 253 sqq.

hour before daybreak on the following morning, when an incident, interesting, as well as important, gave to it a new turn. The ostracized Aristeidēs arrived at Salamis from Aegina. Since the revocation of his sentence—a revocation proposed by Themistoklēs himself—he had had no opportunity of revisiting Athens, and he now for the first time rejoined his countrymen in their exile at Salamis; not uninformed of the discussions raging, and of the impatience of the Peloponnesians to retire to the Isthmus. He was the first to bring the news that such retirement had become impracticable from the position of the Persian fleet, which his own vessel in coming from Aegina had only eluded under favour of night. He caused Themistoklēs to be invited out from the assembled synod of chiefs, and after a generous exordium wherein he expressed his hope that their rivalry would for the future be only a competition in doing good to their common country, apprised him that the new movement of the Persians excluded all hope of now reaching the Isthmus, and rendered further debate useless. Themistocles desired Aristeidēs to go himself into the synod and communicate the news; for if it came from the lips of Themistoklēs, the Peloponnesians would treat it as a fabrication. Thus Grote narrates, after Herodotus, the extremely dramatic meeting of the two rivals. We must indeed modify the statement of the revocation of the sentence of Aristides—which Grote does not derive from Herodotus—so far as, in accordance with the *Ἀθηναίων πολιτεία* (and Plutarch, *Arist.* 8), to speak rather of the revocation of the sentences of all ostracized persons.

This incident is one of those excellent stories of Herodotus, in reading which one cannot forbear entertaining the suspicion that they are incidents which ought to have occurred if real life were only artistic, but which, since real life is nothing if not inartistic, never did. One wonders why Aristides did not return before. The lateness of his return can only be explained by the assumption of some distant place of exile, like Sicily, and if he had gone to Sicily we should probably have heard of it. But it certainly was a very remarkable coincidence that the earliest opportunity of return for him was on the eve of Salamis, an opportunity which enabled him to have a dramatic meeting with his rival and achieve a sensational appearance before the Synedrion. It should be observed that Grote's words 'he had had no opportunity of revisiting Athens' do not express

a direct statement of Herodotus but only a natural, if not necessary, inference from the story. And we should have no alternative but (with or without mental reserves) to accept the story, as one of those rare cases in which history has trespassed on the domain of fiction and created an artistic situation by means of an improbable coincidence, if it were not for a fact in the subsequent narrative which supplies an objective justification of our suspicions.

§ 4. We must go back to the moment at which the Greek fleet, having received the tidings of the disaster of Thermopylae, arrived in the Saronic gulf. The Athenians had to take hasty measures for their own safety, since the confederate army of the Peloponnesians was at the Isthmus and the invasion of Attica was imminent. Herodotus (8, 41) says that a public proclamation (*κήρυγμα*) was made to the effect: *Ἀθηναίων τῇ τις δύναται σώζειν τέκνα τε καὶ τοὺς οἰκέτας*. The *Constitution of the Athenians* (23) supplements this brief statement by the perfectly credible notice (repeated by Plutarch) that the Areopagus assisted the citizens when leaving Attica for places of refuge by a distribution of eight drachmae a head. But it adds the improbable suggestion that the Stratēgoi were unequal to the occasion and that the council of the Areopagus took in hand the organization of the general embarkation. Plutarch (and his source is supposed to be an *Atthis*) speaks of a psephism proposed by Themistocles: *τὴν μὲν πόλιν παρακαταθέσθαι τῇ Ἀθηνᾷ τῇ Ἀθηναίων μεδούσῃ, τοὺς δ' ἐν ἡλικίᾳ πάντας ἐμβαίνειν εἰς τὰς τριήρεις, παῖδας δὲ καὶ γυναῖκας καὶ ἀνδράποδα σώζειν ἕκαστον ὡς δυνατόν* (*Themist.* 10).

The statements of Herodotus and Plutarch are of course quite compatible. The Ecclesia passed a psephism, in consequence of which a public proclamation was made. And the last clause in Plutarch's description of the contents of the psephisma is identical in sense with the effect of the proclamation. Herodotus however says nothing of the clause *τοὺς ἐν ἡλικίᾳ πάντας ἐμβαίνειν εἰς τὰς τριήρεις*. It is important to consider the full bearing of this clause. The transportation of households and property to various places of refuge—Salamis, Aegina, Troezen—is quite clear; but can it really have been that all the able-bodied men served on shipboard? This is evidently what Plutarch meant, and is illustrated by the story of Cimon dedicating his bridle on the Acropolis (*Cimon* 5). Modern historians have not questioned the statement. 'By the most strenuous efforts,' says Grote, 'these few important days were

made to suffice for removing the whole population of Attica—those of military competence to the fleet at Salamis¹—the rest to some place of refuge—together with as much property as the case admitted.'

But it is extremely difficult to take the statement literally. The Athenian triremes were already manned; and it is impossible to suppose that the fixed number of men (two hundred) in any trireme could have been seriously increased, with advantage, or without detriment, to the efficiency of the vessel in a naval action. But allowing that a certain number of recruits might have supplemented losses sustained at Artemisium and even increased by a small addition the regular crew of each trireme, it can hardly be questioned that the number of those who 'went on board the triremes' for the first time at Salamis, was a minority of *οἱ ἐν ἡλικία πάντες*. We may say with certainty that the land army—for though Athens had thrown her main strength into the navy she still had a land force, that which afterwards fought at Plataea—did not, as a whole, embark. This conclusion is confirmed by another consideration. A part of the refugees carried their households to Salamis, and this circumstance implies that some measures beyond the proximity of the fleet, which might be obliged to leave its position in the Salaminian bay or might be defeated, were taken for the defence of that island. And as a matter of fact we find, in the account of the battle, that there were hoplites posted on Salamis (Herodotus 8. 95), to whom I will presently return. We may therefore conclude that, although some men may have been taken from the army for naval service, yet the hoplite force as a whole was not broken up. It is not difficult to account for the phrase in Plutarch, without disputing that his authority genuinely intended to give the purport of an actual decree. The decree probably said in so many words that the whole population was to embark, in order to be removed to the various places of refuge. There is every reason to suppose that the fleet was used for the purpose of removal. This general embarkation, combined with the fact that the army played little more than the part of a spectator at Salamis and was quite in subordination to the fleet, created the idea that all able bodied Athenians fought on shipboard at Salamis.

¹ These words are in themselves ambiguous, not necessarily meaning service on shipboard; but this is accidental, for Grote had told the story of Cimon and gives no hint that he does not adopt the usual view.

It is an idea however that we do not find in Herodotus.

§ 5. If I may be allowed to turn aside for an instant from my immediate purpose, the question may be asked whether, as we have found hoplites in Salamis, all the hoplites (apart from any who did take service in the navy) were posted there. An affirmative answer would have to be given, if it were certain that Athens had been utterly and absolutely abandoned. But this seems to me very far from certain, and on the contrary it may, I think, be maintained that a small part of the Athenian army was left at Athens. To show this, the story of Herodotus must be examined.

The Persians, we are told (8, 51) when they arrived in the city, found it deserted, save for a few people, the *Tamiae* of the Temple of Athena, and some poor men, on the acropolis. These few men gave the Persians much trouble and held out *ἐπὶ χρόνον συγχρόν*—an expression which from other notes of time has been reckoned to represent about a fortnight.² If a few *πένητες ἄνθρωποι* could defy the forces of Xerxes so long, the Athenian generals might well be asked whether they were wise in abandoning such a strong position as their citadel. Defended by a properly organized garrison, might it not have successfully withstood all attempts of the Persians to take it, until it was relieved through a naval victory?

Herodotus himself gives us the means of criticizing his story, and without design discloses the truth. We are surprised to read that, when the Greeks at Salamis heard of the capture of the Acropolis, they fell into great consternation; *ἔς τοσοῦτον θόρυβον ἀπίκοντο, ὥστε ἐνιοὶ τῶν στρατηγῶν οὐδὲ κυρωθῆναι ἔμενον τὸ προκείμενον πρῆγμα, ἀλλ' ἔς τε τὰς νέας ἐπέπιπτον καὶ ἰστία ἡείροντο ὡς ἀποθνεύσόμενοι*. But if the Acropolis was abandoned and left without any defence, save that of a few poor or eccentric people who chose to remain with the *Tamiae* of the Temple, it is clear that its capture must have seemed a foregone conclusion. The utter consternation of the Greeks is inconsistent with the narrative which represents the citadel as left without deliberate defence. The inference is that the Athenian generals placed a garrison in the Acropolis, and that the tale told by Herodotus is only a tale.

And it is a tale of which the origin can be analyzed. It is an example of history reconstructed on oracles, which were themselves constructed on history.

² Busolt 2, 694.

Herodotus relates (7, 140, 141) that the Athenians sent to consult the Delphic oracle. The answer—bidding them flee to the ends of the earth and ending with the verse

ἀλλ' ἔτον ἐξ ἀδίτοιο κακοῖς δ' ἐπικίδνατε θυμόν—

was so disheartening that they asked a second time, in the posture of suppliants, and received the following oracle :—

οὐ δύναται Παλλὰς Δέ' Ὀλύμπιον ἐξίλασθαι
 λισσομένη πολλοῖσι λόγοις καὶ μῆτιδι πυκνῇ·
 σοὶ δὲ τόδ' αὖτις ἔπος ἔρέω, ἀδάμαντι
 πελάσσας·
 τῶν ἄλλων γὰρ ἀλίσκομένων, ὅσα Κέκροπος
 οὔρος
 5 ἐντὸς ἔχει κευθμών τε Κιβαιρῶνος Ἰαθεοῖο,
 τεῖχος Τριτογενεῖ ξύλινον διδοὶ εὐρύσπα Ζεὺς
 μούνον ἀπόρθητον τελέθειν, τό σε τέκνα τ'
 ὀνήσει.
 μηδὲ σὺ γ' ἱπποσύνην τε μένειν καὶ πεζὸν
 ἰόντα
 πολλὸν ἀπ' ἡπείρου στρατὸν ἥσυχος, ἀλλ'
 ὑποχωρεῖν
 10 νῶτον ἐπιστρέψας· ἔτι τοί ποτε κἀντίος
 ἔσση.
 ὦ θεή Σαλαμῖς, ἀπολεῖς δὲ σὺ τέκνα
 γυναικῶν
 ἣ πον σκιδναμένης Δημήτερος ἢ συνιούσης.

It has been long recognized that the last two lines were composed *ex eventu* (cp. Wilamowitz-Möllendorff, *Kydatheon*, p. 97); but we must apply the same principle to vv. 8–10, the words *ἔτι τοί ποτε κἀντίος ἔσση* containing a manifest allusion to Plataea. On the other hand there is no reason for doubting that the Athenians consulted the oracle,—after the disaster of Thermopylae, of course, and not before the beginning of the war, as is suggested by the place (before the Isthmian congress) in which the episode is introduced by Herodotus, though after his manner he gives no express chronological indication.

We may accept, without difficulty, the first seven lines as the actual utterance of the Delphic oracle shortly before the battle of Salamis. But we must read them as intended by the Delphic priesthood to be capable of the interpretation which Themistocles gave. We must place ourselves in the position of the Athenian government. The wise policy, on which they resolved, of moving the whole population of Attica was a policy of which the execution was obviously attended by great difficulties and likely to meet with considerable and possibly obstinate resistance from a large part of the people.

In such a case, there was one step which a prudent government could not neglect, namely, to enlist the support of the Delphic oracle and strengthen their policy by an appeal to the authority of the god. The oracle which Herodotus records, shorn of its later additions, is, to all appearances, the result of an understanding between the Athenian government and Delphi. The priesthood, of course, in their usual method safeguarded the god by using an ambiguous phrase—*τεῖχος ξύλινον*—, which, in case the policy recommended by the Athenian government proved disastrous, admitted of other interpretations, for instance that of 'some of the older men' who thought that the Acropolis was meant (c. 142). But the oracle loses its significance so long as it is not recognized that it is the answer of Apollo to Themistocles and the Athenian government seeking Delphic support for a particular policy.

The strength of the Acropolis—the event proves how strong it was—almost forbade the idea of abandoning it without an attempt to defend it. And the ambiguity of the oracle was an additional reason. For the most convincing answer to those who referred the oracle to the Acropolis was 'But in any case we are taking measures to defend it.' In this way both of the rival interpretations would be satisfied. Afterwards, when the Acropolis had failed *ἀπόρθητον τελέθειν* and when the policy of the government had been strikingly approved by fortune, the history of the events was recomposed with regard to what was now recognized universally as the true meaning of the oracle. The unsuccessful defence of the Acropolis was represented as the act of a few poor insignificant people and not a deliberate and organized military resistance.

§ 6. Respecting, then, the disposition of the Greek army at the time of Salamis, it emerges from this discussion, that, while a few hoplites were probably transferred for naval service, a distinct detachment was deputed to garrison the Acropolis, and the remainder, by far the greater part, was posted in Salamis. There were ten *Stratēgoi*, some of whom, along with the chief *Stratēgos* Themistocles, commanded the ships, but some—at least one—must have been in command of the hoplites on the island of Salamis. It was their—or his—business, on the day of the battle, to act according to the fortunes of the fleet, and take defensive or offensive measures according to the exigency of the case. As it turned out,

offensive action was called for, and such action on the part of the hoplites is duly recorded by Herodotus. They crossed over from the shore of Salamis to Psyttaleia and slaughtered all the Persians who were on the islet. But we are astonished to read that the hoplites act not under the direction of a *stratēgos* but under the command of a private person, the ostracized Aristides, who had returned from banishment only the night before.

There is a manifest difficulty in reconciling this incident with the dramatic episode of the first appearance of Aristides on the eve of Salamis. One could readily understand a private person of influence and energy gathering a number of volunteers for some patriotic service at a critical moment, but one cannot easily conceive a private person usurping the functions of the *Stratēgos* over a portion of the army.

The simple solution is that *Aristides was himself one of the Stratēgoi*. Herodotus did not apprehend this, and, although he nowhere says expressly that Aristides returned from exile on the eve of Salamis, his account of the interview between Aristides and Themistocles most readily lends itself to such a reading. All the facts are true—the fact that Aristides brought the news that the Greeks were surrounded, and the fact that he managed the affair of Psyttaleia. But the suppression of the fact that he was *Stratēgos* has made it possible to represent him as reappearing for the first time at the *Synedrion* of the generals on the eve of Salamis.

§ 7. But if Aristides was *Stratēgos*, how came it that he crossed over from Aegina (ἐξ Αἰγίνης διέβη) on the night before the battle? We have here an illustration of the disconnected nature of the sources from which Herodotus drew his material. If Aristides was a *Stratēgos* his absence at this crisis must have been for the purpose of some public service. Now Herodotus records that a trireme had returned from Aegina, before the battle began (viii. 83 καὶ ἦκε ἡ ἀπ’

Αἰγίνης τριήρης)—the trireme which had been dispatched to bring the Aeacids (c. 64). The obvious conclusion is that this was the ship in which Aristides crossed over from Aegina, and that he had been deputed to take charge of the mission to bring the Aeacids.

§ 8. It is to be observed that this hypothesis does not contradict any statement of Herodotus. That historian nowhere says that the *diabasis* of Aristides from Aegina was his first return to his country. Nor is the fundamental importance of the dialogue between Themistocles and Aristides abolished, although its dramatic effect is weakened. The significance of that dialogue still remains, assuming, however, the shape of a hearty cooperation between two *Stratēgoi* at the *Synedrion* in which both—Aristides as well as Themistocles—were entitled to take part. It may be added that the hypothesis is confirmed by the political wisdom of reconciling the ostracized statesmen on their return by entrusting to them at once posts of importance. We may guess¹ that Xanthippus too was one of the ten *Stratēgoi* in the autumn of 480 B.C., and that when in the spring of the following year he acted as chief admiral in place of Themistocles, he was not elected as a new *Stratēgos* but was raised from a subordinate to the chief place in that portion of the Strategic board which was concerned with the fleet. In any case Aristides retained his *Stratēgia* throughout the campaign of the following year, and, as the land army was then acting independently of the fleet, he played a part of greater prominence than he was allowed to play at Salamis.

¹ The anecdote of his dog, left behind on the Attic coast and drowned in an attempt to swim across to Salamis, suggests that Xanthippus was remembered in connection with the removal of the Attic population before the battle, and raises the presumption that he took part in organizing that removal, and therefore that he held a public office, which may have been that of *stratēgos*.

J. B. BURY.

THE CORINTHIAN CONSTITUTION AFTER THE FALL OF THE CYPSELIDES.

THE constitution of Corinth established after the expulsion of the tyrants is thus described by Nicolaus Damascenus (Müller, *F.H.G.* fr. 60): αὐτὸς δὲ (sc. ὁ δῆμος) παραχρῆμα πολιτείαν κατεστήσατο τοιάνδε· μίαν

μὲν ὁκτάδα προβούλων ἐποίησεν, ἐκ δὲ τῶν λοιπῶν βουλὴν κατέλεξεν ἀνδρῶν θ’.

This passage has given scholars a great deal of trouble. And, indeed, the number of members of the βουλὴ indicated in the

text is evidently wrong. But the thought itself will be clear as soon as we cease to hold the prejudicial opinion that *ὀκτὰς* can mean only the number 8. That is the common use, to be sure. But Nicolaus was not so good a writer that he might not have sometimes departed from the pure style. *Ἑβδομῆς* is not always a period of seven days, it may also be the seventh part of this period. Why might not Nicolaus, being an Hellenistic writer, have used *ὀκτὰς* in a similar way? *Ὀκτὰς* is, I believe, in this connection the eighth part of a whole i.e. one of the eight *φυλαί*, into which the citizens of Corinth were divided. The meaning is this: the populace made one of the eight *φυλαί* the *φυλή* *προβούλων*, i.e. the council of the *πρόβουλοι* had to be taken from this *φυλή*. From the other seven *φυλαί* the *βουλή* was chosen.

What was the character of this constitution, what the power of these two bodies? Aristotle will help us to answer these questions. Pol. 11299b (Susemihl) he says: *ἀλλ' ὅπου ἀμφω αἰτῶν αἱ ἀρχαί, οἱ πρόβουλοι καθεστῶσιν ἐπὶ τοῖς βουλευταῖς. ὁ μὲν γὰρ βουλευτῆς δημοτικόν, ὁ δὲ πρόβουλος ὀλιγαρχικόν, i.e.* 'If the *πρόβουλοι* and the *βουλή* exist side by side in the same state, the *πρόβουλοι* have authority over the *βουλή*; for the *βουλή* is a democratic, the *πρόβουλοι* an oligarchic power.'

Another passage of the same writer (1298b) is this: *ἐν δὲ ταῖς ὀλιγαρχίαις ἡ προαιρέσθαι (sc. συμφέρι) τινὰς ἐκ τοῦ πλήθους ἢ κατασκευάσαντας ἀρχέον οἷον ἐν ἐνιαυτοῖς πολιτείας ἐστὶν οὗς καλοῦσι προβούλους ἢ νομοφύλακας [καὶ] περὶ τούτων χρηματίζειν περὶ ὧν ἂν οὗτοι προβουλευσώσιν (οὕτω γὰρ μεθέξει ὁ δῆμος τοῦ βουλευέσθαι καὶ λύνει οὐδὲν δυνήσεται τῶν περὶ τὴν πολιτείαν), ἔτι ἢ ταῦτα ψηφίζεσθαι τὸν δῆμον ἢ μηδὲν ἐναντίον τοῖς εἰσφερομένοις, ἢ τῆς συμβουλῆς μὲν μεταδιδόναι πᾶσιν, βουλευέσθαι δὲ τοὺς ἀρχοντας, καὶ τὸ ἀντικείμενον δὲ τοῦ ἐν ταῖς πολιτείας γινόμενον δεῖ ποιεῖν [τὸ πλήθος], ἀποψηφίζόμενον μὲν γὰρ κύριον [εἶναι] δεῖ ποιεῖν τὸ πλήθος, καταψηφίζόμενον δὲ μὴ κύριον, ἀλλ' ἐπαναγέσθαι πάλιν ἐπὶ τοὺς ἀρχοντας [ἐν γὰρ ταῖς πολιτεῖαις ἀνεστραμμένως ποιοῦσιν, οἱ γὰρ ὀλίγοι ἀποψηφισάμενοι μὲν κύριοι, καταψηφισάμενοι δὲ οὐ κύριοι, ἀλλ' ἐπαναγεται εἰς τοὺς πλείους αἰε].* The text is evidently corrupt. But by dropping *καὶ* before *περὶ τούτων χρηματίζειν* Susemihl has not remedied the fault. The opposite to *προαιρέσθαι τινὰς* is clearly expressed by *ἢ τῆς συμβουλῆς μὲν μεταδιδόναι πᾶσιν*. From this it follows that the *ἢ* standing before *κατασκευάσαντας* has no place here. By dropping it instead of *καὶ* before *περὶ*

τούτων χρηματίζειν the whole period will be divided into two almost equal members. Treating the question how a pure oligarchy could be improved by means of an admixture of democratic institutions Aristotle indicates two methods. The one is to commit the final decision of public matters to a select part of the common people, but to restrict the supremacy of this body by the oligarchic power of the *πρόβουλοι*; the other, to make the whole body of citizens participants in public deliberations, but to give them only the right of counselling. The latter method has nothing to do with our subject, but the first undoubtedly concerns the constitution of which we are now treating. The *βουλή* mentioned by Nicolaus and the select body of commons that according to Aristotle has to rule public affairs and to be ruled itself by the council of the *πρόβουλοι* are manifestly the same. When Aristotle says that the first method recommended by him is really used in some states, I have no doubt that he has the Corinthian state in mind as one of them.

Therefore the constitution of Corinth is presumably this. The whole of the citizens are divided into eight *φυλαί*, one of which contains the nobility, the rest the common people. It is not impossible that this division instead of that into the three old Doric tribes took place at the time of the new organization of the state. Public affairs are ruled by two bodies, one of which is taken from the nobles, the other from the people. The one, named *πρόβουλοι*, has the right of the first deliberation in any public matter; the other named *βουλή*, the final decision of the propositions introduced by the council of nobles. The rights of the *βουλή*, having some appearance of sovereignty, are limited in two ways. No bill refused by the *πρόβουλοι* can be discussed by the *βουλή* and if the *βουλή* resolves differently from the *πρόβουλοι*, its resolution can be annulled by the latter. Therefore the supremacy of the commons, restricted in the two most important ways, is mere show. If they possess any real right at all, it is only that of vetoing laws; for it seems probable that the measures of the *πρόβουλοι*, if rejected by the *βουλή*, could not have the force of laws.

So the power of the nobles, without slipping out of their hands, rests upon a broader basis, a fact to which is undoubtedly due the well-known strength and permanence of the Corinthian constitution.

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GREEK METRICAL INSCRIPTIONS FROM PHRYGIA.

THE following inscriptions are edited from epigraphic copies given me by Prof. W. M. Ramsay, to whom my best thanks are due for help and advice.

I.

Found at Utch-Eyuk, in the country of the Praipenisais (Ramsay, *Histor. Geogr. of As. Min.* pp. 144 f.)

- Αὐρ. Μένανδρος Πρόκλου
 κὲ Ἀππης τέκνη φιλάτω Πρόκλω
 κὲ ἑαυτοῖς ζῶντες κὲ τὰ τέκνα
 αὐτῶν Τρόφιμος κὲ Μέναν[δρ]ος
 5 κὲ Κύριλλα κὲ Δόμνα νύμφη
 μνήμης χάριν Τατιανῆς [θ]υγάτηρ.
 φαιδρότατον βωμὸν στήσαι σημάντορα τύμ-
 βον,
 εὐτ' ἂν ὕδωρ τε ῥέει κὲ δένδρεα μακρὰ τεθήλη,
 κὲ ποτ[α]μο[ι] ναίουσιν, ἀνα[β]ρύζη δὲ
 θά[λασ]σα.
 10 αὐτῷ τῷδε μένω πολυκλαύτῳ ἐπὶ τύν[β]ῳ.
 ἀγγέλλω παριοῦ[σ'] ὅτι Πρόκλος ὧδε τέ-
 θ[απτ]αι.
 πᾶσι ποθητὸν ἔοντα κὲ ἐν βίῳ πανάριστον
 οἰκίῳ ἐλιπον φάος, αἶψα δὲ μο[ι]ρα
 καρπαλίμως ἐ[δ]άμασσε κὲ αἶσα λυγρ[ή]
 ἐ[π]όρουσε.
 15 αὐτὸς δ' Ἐννοσίγαιος ἔχων χεῖρεςσι τρίαῖναν
 κτείνει με τὸν μέλεον Τενβρογγ[ί]ου παρὰ
 ῥεῖθρα

1. Αὐρ. is Αὐρήλιος, a common *praenomen* in the third century. 2. Ἀππης is a noticeable form of female name. 5. νύμφη cf. Τενβρογγίου (l. 16). 6. Τατ. θυγ. is evidently a designation of Δόμνα νύμφη, misplaced by engraver. 7. στήσαι may be taken either as inf. for imper., or as imper. aor. mid. -ον of βωμὸν is repeated by engraver's error on stone, as is also -τω of πολυκλαύτῳ (l. 10). 8. εὐτ' ἂν is followed irregularly by indicatives and subjunctives. 13. There is a foot too few, read perh. [ε]κ[οσι καὶ δὲ] ἑ[πτά]ων. W.M.R. The κ would itself represent εἰκοσι. οἰκίῳ is hardly likely. 14. λυγρῇ has the ν short. In this word it is almost invariably long. 1. 2 of Inscr. iii. is another exception. 15. Stone has ἐν χεῖρεςσι, one syllable too many. Alter as above, or to ἐν χερσί. 16. The river Tembrogius (modern *Porsuk Su*) is mentioned by Pliny (*H. N.* vi. cap. 1):—*'Sagaris fluius ex inclutis: oritur in Phrygia, accipit uastos amnes, inter quos Tembrogium et Gallum.'* It is called Tem-

bris on coins (cf. Waddington, *Voyage Numismatique en Asie-Mineure*, p. 28 ff; Ramsay, *Hist. Geogr. of As. Min.* 144, 178). It is the 'Tymbris' of Livy (xxxviii. 18). Another form *Thybris* occurs: v. Nicetas Chon., p. 89, and Cinnamus, p. 81, 191.

All the names of persons in this inscr., except Πρόκλος, appear in an inscr. found at Kotiaion (C.I.G. 3827r, Le Bas-Wadd. 821). This must be more than a curious coincidence, and surely proves relationship.

There seem to have been models in currency for epigrams such as the present. Line 15 is *Il.* M. 27. καρπαλίμως and ῥεῖθρα are Homeric words and in Homeric position, though the latter is usually ῥεῖθρα: φαιδρός, ποθητός, and πανάριστος are, however, un-Homeric. Words like Τενβρογγίου are foisted in to suit the occasion, and have the uncouth appearance of new stones in an old building. The special interest of this epigram, however, lies in ll. 8–11. Their intimate relation to four lines in the 'Homeric' epigram Εἰς Μίδην (last edited in Mr. D. B. Monro's *Homer* pp. 999–1000), is all the more interesting from the fact that this stone was found near the tomb of Midas. The differences in text must be noted:—ἐς τ' ἂν for εὐτ' ἂν, πλήθωσιν for ναίουσιν, ἀνακλύζη for ἀναβρύζη, and μένουσα for μένω (due to χαλκή παρθένος in l. 1). Moreover l. 3 of the Homeric version is omitted here. Another version, quoted probably from memory, is to be found in Plato, *Phaedr.* 264 D, where ll. 3–4 of the epigram are omitted, and in which the following differences may be mentioned:—ὄφρ' ἂν for ἔστ' ἂν, νάη for ῥή (cf. ναίουσιν of l. 9) and, πολυκλαύτου ἐπὶ τύμβου for πολυκλαύτῳ ἐπὶ τύμβῳ.

II.

Found at Dokimion.

† νωνυμῖν φορέοντα, ἀτερπέα χώρον ἔοντα, γονν[όν] κ[ε]κμηῶτα, [ἐ]οῖς κτεάτεσσιν ἐγ(ε)ῖρας, Εὐνόμιος κόσμησε, Σατορνέ[νοιο] γενέθλη, εἰσορό[ων] μετὰ πάντα πέλειν καὶ τοῖδ' ἀλε-
 [ωρή]ν.

1. νωνυμία is cited by Liddell and Scott only from Hesychius. ἀτερπέα χώρον is from *Od.* λ 94. 3. Eunomios, son of Saturninus, is probably identical with the Eunomios of C.I.G. 9267 (Le Bas-Wadd. 1714), who re-

stores the tomb of an ancestor, an ancient bishop of Dokimion. 4. is difficult.

III.

Found at Uch-Kuyu.

ἐννέα καὶ δεκετῇ Τάταν ἐν τῷδε [ε τῷ] τύνβῳ
θῆκα γονεὺς ὁ λυγρὸς καὶ ἡ μήτηρ βαρυπενθής.
πέντε δὲ μῆνας ἀνδρὶ συνοικίσασαν νέαν νέψ
ἐφθάσε μοῖρα.

τοΥΓ ΝΙΟΙ . . . ὁμων θάνατον
αὖ

1. Τάταν spondaic: cf. Τάριανός C.I.G. 6274, and the form Τατῆς, C.I.G. 4321b, 4341e. The name is the same as Tottes (cf. Tataion = Tottaion, Ramsay, *Hist. Geogr. of As. Min.*, 18, 437, 439), and the original meaning is evidently 'father.' The ending of this line is weak. 2. λυγρός, cf. Inscr. i. l. 14, note. 3. ἐφθάσε μοῖρα is evidently the ending of this line, ἀνδρὶ σ. ν. νέψ being an unmetrical insertion. 4. ΝΙΟΙ part of οὐράνιος?

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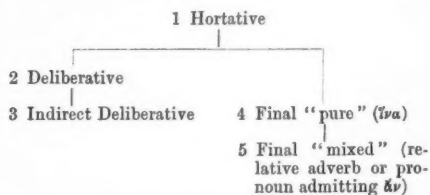
(To be continued).

OF THE SUBJUNCTIVE IN RELATIVE CLAUSES AFTER οὐκ ἔστιν AND ITS KIN.

IN the last volume (VII.) of the *Harvard Studies in Classical Philology* the first place (pp. 1—12) is occupied by an article by Professor William W. Goodwin entitled *On the Extent of the Deliberative Construction in Relative Clauses in Greek*. This paper reviews in part the discussion started by Mr. Arthur Sidgwick in the *Classical Review* of April, 1891, and also sets forth Mr. Goodwin's latest view of the matter. I have been prompted to write what follows by the fact that Mr. Goodwin takes no notice of a theory broached by me in *Some Remarks on the Moods of Will in Greek* which appeared in the *Transactions of the American Philological Association* for 1895 (vol. XXVI., *Proceedings of the Special Session*, 1894, pp. 1.—li.) but credits me with a view of the subject of the discussion that I have expressly abandoned. It is with a certain hesitation and regret that I thus express my disagreement on an important matter of Greek syntax with one to whom I—like so many others—owe the first impulse to the study of Greek syntax; but I venture to do so at once in justice to myself and with a desire to contribute to the ascertainment of truth in regard to the debated construction. I begin with a brief discussion of certain of Mr. Goodwin's statements.

At p. 1 Professor Goodwin speaks of the clauses in question as seeming 'to lie in the borderland between indirect deliberative questions and final relative clauses.' Now both the indirect deliberative question and the final relative clause are 'subjunctive' developments of the primitive 'hortative.' Thus the 'hortative' ἴωμεν *let's go*—I use the

colloquial form to distinguish the exhortation from the appeal—becomes, when treated as an interrogation, ἴωμεν; *shall we go?* in which the question is put (and this is to be emphasised) to the subject of the verbal form minus ἐγώ, the action being at the same time conceived as to be performed by the entire subject, ἡμεῖς. This interrogative ἴωμεν; may, of course, be subordinated (indirect deliberative question). The 'final' clause, whether of the ἴνα type or of the relative pronominal type, subordinates, or makes a 'subjunctive' properly so-called, of ἴωμεν *let's go*. The pedigree of the divergent uses of the same verbal form may be indicated thus:—



Mr. Goodwin's 'borderland' lies between 3 and 5 and is, as appears in his subsequent discussion, a territory of analogy—whether true or false is beside the question.

I have been at pains thus plainly to set forth the genealogy of these uses because some of the disagreement among those that have engaged in the discussion I conceive to be due to the disregarding or ignoring of the steps in the development of the several uses of what we call collectively the subjunctive. That I have been guilty of the

fault of which I venture to accuse others I have elsewhere (*Transactions* 1895, *loc. cit.*) admitted; and I here again concede that in claiming that I was in error in seeking to derive the form of clause in question from the relative clause of purpose Mr. Hale is entirely in the right—, and that too although I do not admit the truth of all that Mr. Hale has said in his 'Extended' and 'Remote' *Deliberatives in Greek* in refutation of my former position. But it is not my intention to deal now (if ever; for we differ, e.g., *toto caelo* in our understanding of the primitive force of the subjunctive) with Mr. Hale's arguments. It is, after all, of little moment in the case at issue to discuss the legitimacy of the steps by which the falsity of a position that one has taken up has been shown, if one but admit the falsity. But to return to Mr. Goodwin's paper.

At p. 2 Mr. Goodwin gives as types of the construction in question the following:

ἔχειν ἐφ' οἷς φιλοτιμηθῶσιν,
Isocr. iv. 44.

οὐκ ἔχω σόφισμ' ὅτ' ἀπαλλαγῶ,
Aesch. *Prom.* 470.

οὐδένα εἶχον, ὅστις ἐπιστολὰς πέμψει,
Eur. *I.T.* 588.

I may be pardoned if I anticipate the statement of my own theory so far as to call attention to the fact that Mr. Goodwin gives here only clauses dependent upon a form of ἔχειν and none that depend upon a form of εἶναι; for it is at this point that we part company.

At p. 3 Mr. Goodwin says: 'It is generally admitted—that the same deliberative interrogative may follow οὐκ ἔχω in the sense of ἀπορῶ, as in οὐκ ἔχω ὅ τι εἶπω, *I have nothing to say*; where, however, the English translation is misleading, the literal meaning being *I have not* (i.e. *I am at a loss*) *what I shall say*. That ὅ τι is really interrogative here is plain from cases like οὐκ ἔχω τί λέγω, *I have nothing to say*, Dem. ix. 45; οὐκ ἔχω τί φῶ, Aesch. *Cho.* 91 and οὐκ ἔχω ἐπὶ τίνα μολοῦνταν πορευθῶ, Eur. *Alc.* 120; and this appears in the Latin *non habeo quid* (or *quod*) *dicam*.' Here I cannot but think that he falls into error. Although Mr. Hale seems more than inclined (*Transactions Am. Philol. Assoc.* 1893, p. 161 sq.) to call me to task for assuming that the ambiguity of ἔχειν (*have, know, be able*—the last meaning playing no part in the present discussion) and of ὅστις (ὅς + τις, and also—according to Greek feeling, I am more than inclined to

think—ὅς + τις; = τις; in indirect questions) has been ignored, I can not but think that what I wrote then (*Class. Rev.* 1892, p. 94) was fairly justified. Does not the fact that the simple interrogative does not (certainly) appear in any of the examples of the construction in question, whereas the compound ὅστις or the simple ὅς is used in the debated construction (though also in the indirect interrogative clause), shew that the Greeks distinguished, to a certain and very considerable extent, between the meanings *have* and *know* in ἔχειν? Mr. Goodwin's translation of οὐκ ἔχω ὅ τι εἶπω, when οὐκ ἔχω = ἀπορῶ, should not, I must believe, be "*I have nothing to say*" but *I have no knowledge what I am to say*. The same remark applies to οὐκ ἔχω τί λέγω. For a similar reason it appears wrong to state the Latin form as if *quod* were a mere variant of *quid*.

Mr. Goodwin is hardly fair to himself when he speaks of his 'uninstructed mind' (p. 3). The seemingly spontaneous feeling of a mind fit for and trained to the consideration of niceties of expression may be nearer right than the δεύτεραι φροντίδες. I am sorry that Mr. Goodwin regrets my 'bringing up in judgment against him' his note of 1863; but then he has brought up in judgment against me opinions that I have expressly modified (*Transactions*, 1895, *loc. cit.*).

I should anticipate too much of my own theory (only a restatement, after all), were I to take up the affirmative forms ἔχειν ἐφ' οἷς φιλοτιμηθῶσιν etc. at this point. Their explanation follows from, or better, goes hand in hand with that of the negative form.

The example from Plato's *Ion* (discussed pp. 3 and 4) proves what the forms of expression used in the debated construction prove elsewhere, viz. that the Greeks did not hold the relative and the interrogative sharply asunder at all stages of their development. It does not prove that the two expressions are to be explained as steps in one and the same course of development. Secondary contamination does not prove primary community of source.

I need hardly say in respect of the second paragraph on p. 4 that I deny Mr. Goodwin's major premiss that ὅ τι δῶ in the passage in the *Anabasis* is an interrogative clause.

The paragraph beginning 'We have thus come' (p. 4) seems to bring some distant hope of a nearer agreement; for Mr. Goodwin here appeals to the force of the independent interrogative ἔλθωμεν; as the in-

terrogative of the independent hortative ἔλθωμεν.

At p. 6 Mr. Goodwin at length gives what it could be wished that he had given earlier, examples of the debated construction dependent upon a form of εἶναι (Eur. *Orest.* 722 etc.). Curiously, as it seems to me, he treats this formula as a development of the ἔχων formula, not *vice versa*.

At the same page Mr. Goodwin concludes his discussion of the subjunctive *per se* by giving his formal approval to the term 'extended deliberative.' Inasmuch as his subsequent treatment of the optative is directly dependent on his treatment of the subjunctive, I may be permitted to set forth here what I venture to believe to be the true explanation of the construction under discussion,—an explanation at which I have already more than hinted (*Transactions* 1895, *loc. cit.* p. li. top).¹ This brings us back at once to genealogy.

It seems but fair to take as the primitive use of the subjunctive (using the term in its commonly accepted wide sense) that which

¹ I venture to add here in the form of a foot-note remarks on one or two points in Professor Goodwin's treatment of the optative in his paper.

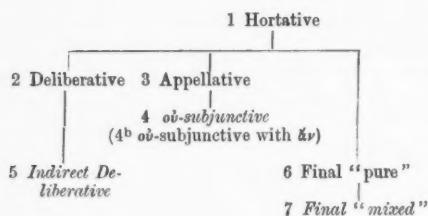
In *Class. Rev.* 1893, p. 451, I have offered an explanation based on analogy—and which I still believe to be correct—of the opt. in Soph. *Trach.* 903.—In Ar. *Ran.* 97 why should λαοὶ not be treated like πέμψετε in Eur. *I.T.* 588? The one verb 'expresses purpose' just as 'clearly'—or *unclearly*—as the other. The φέρεσθαι in the next verse is not unnatural. We pass from a *should* (for a *shall*) utter to a more independent *will* utter. Thus the optative would be due to attraction or assimilation.—Inasmuch as μέλλοι βοηθήσειν (p. 9) = βοηθήσοι, it were better treated simply as a μέλλει βοηθήσειν that has turned optative by assimilation, just as a βοηθήσει might.—After what Mr. Goodwin says about 'a distinct conditional force' in the example just alluded to I will not venture to discuss the reference to my own attitude of mind that he makes in the footnote on p. 10. Our points of view are too widely separate.—It need hardly be said that in discussing Soph. *Phil.* 270-282 I believe Mr. Jebb to have gone too far back when he says that the dependent optatives here represent direct questions (τίς ἀρκέσῃ; and τίς συλλάβηται;). In my view they should rather be treated as optative mutations of οὐτίς ἀρκέσῃ and οὐτίς συλλάβηται in analytic form.—With Mr. Goodwin's remark (p. 11) that 'the difference between δρῶν οὐδένα δοτὶς ἀρκέσῃ and ἔχων οὐδένα δοτὶς ἀρκέσῃ is surely not generic;' etc. (to the end of the sentence) I am in complete accord.—Is not Mr. Goodwin's remark (p. 12) that 'the aorist optative in Dem. vi. 8 seems to come from a tendency to use an optative after the preceding optative and an objection to using the future' somewhat (*mea quidem opinione*, in principle) at variance with what he says in the first paragraph of the footnote to p. 10?—I may be pardoned if I add that I have (or rather, had) 'considered carefully Gildersleeve's wise and acute remarks' (see footnote p. 10) and that I too regard his formula ὅπως ἐν = ἥν πως as 'a powerful solvent.'

is simplest and which has best stood the test of time in independent use, viz. the 'hortative.' Ἱώμεν *let's go* and μὴ Ἱώμεν *let's not go* with their corresponding interrogative use (the 'deliberative') form, as is generally admitted, the basis of many (at least) of the dependent uses of the subjunctive, or, as may well be said, the basis of the 'subjunctive.' But there is another independent use of the verbal type which Ἱώμεν represents besides the 'hortative' and the 'deliberative,'—a use which corresponds to our English *shall*-future. The negative in this case is οὐ not μὴ and the first example is at *Il.* A 262. This usage may be explained as derived from the 'hortative': but there is apparently an intermediate step. In the hortative the subject of the verbal form includes the person or persons addressed by the speaker. So too, when the 'hortative' is used in the singular in communion with one's self. But both the 'hortative' and the 'deliberative' may become, not unnaturally, an 'appellative,' the person or persons addressed being conceived as entirely apart from and external to the subject of the verbal form.

The answer to the 'hortative' is expressed in terms of the 'hortative'; that is to say, either it is a mere echo, if the will of the persons addressed coincide with that of the speaker; or it is the contradictory of the form used by the speaker, if the will of those to whom he addresses himself be adverse. In the case of the 'appellative,' however, the answer is expressed in terms of the imperative. But besides the answer to the appeal we have to consider what I have elsewhere called a 'reflex,' i.e. the verbal expression of the impression that the result of the appeal leaves upon the mind of the appellant. At the place just referred to (*Transactions*, 1895, p. li), after characterizing the subjunctive in general as 'the mood of trammelled effort'—a term of which, it may be added, I believe Mr. Hale approves, I have said: 'the reflex of trammelled effort might well be an expression of resignation—naturally negative. This may explain *Il.* 1, 262.' [Of course, the positive 'reflex,' equally possible, would express what one is to do under the authority or control of persons or circumstances.] 'Should we resort here to the familiar Greek device of emphasizing the negation by making it a separate sentence, we should expand this passage to οὐ γὰρ πω—οὐδ' ἐστὶν ὅπως ἰδωμαι. We shall thus have traced to its origin a form of expression that has given much trouble.' This

view of the construction in question I still hold, although I use the term appeal to cover the interrogative form as well as that used in the illustration that I have employed in the passage just quoted. This 'οὐ-subjunctive,' to give it its conventional name, may take ἄν like the 'οὐ-optative.' (How far this use of the particle with the 'οὐ-subjunctive' may have affected, if at all, the subjunctive in 'relative final clauses' is a question that no man can answer. A certain amount of contamination is, of course, possible.)

I would now draw up another pedigree, thus:—



The theory that I have abandoned would derive the subjunctive in the clause dependent on οὐκ ἔστιν (οὐκ ἔστι μοι, οὐκ ἔχω: for so I would evolve the common form of the introductory sentence) from 7; the theory of Mr. Hale accepted by Mr. Goodwin would derive it from 5; the theory held here and in the *Transactions* for 1895, would derive it from 4. I may add, without in any way abandoning my position, that the persistence in Attic Greek of this derivative of 4 at the expense of the derivative of 4^b (with ἄν) may be explained by the formal influence of 5 upon 4.

Though Mr. Goodwin has not in the paper that I have just examined treated the optative without ἄν in relative clauses dependent upon οὐκ ἔστιν and οὐκ ἔχω in the present, I may add that it follows as a corollary from the theory just set forth in respect of the subjunctive that this remarkable optative in Attic Greek is a survival of the οὐ-optative. The noteworthy sequence marks it as archaic and archaistic.

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THE DATIVE SINGULAR OF THE FIFTH DECLENSION IN LATIN.

WE are accustomed to regard the genitive and dative singular of the fifth declension as similar in form, e.g. *faciēi, fidēi*, and to compare them in this respect with the gen. and dat. sing. of the first declension. And we have the authority of Priscian for so doing. Priscian, writing in the sixth century A.D., says (i. p. 366 H.): *genetivus et dativus ejus declinationis sunt similes; fiunt enim extrema s nominativi abjecta et assumpta i: ut 'hic dies,' 'hujus diei,' 'huic diei'; 'haec facies,' 'hujus faciei,' 'huic faciei.'* Et servant quidem productionem nominativi, si i habeat paenultimam, ut 'acies, aciei,' 'rabies, rabiei'; sin autem consonantem habeat ante -es, corripitur e tam in genetivo quam in dativo, ut 'haec fides, fidei,' 'res, rei,' 'spes, spei,' 'plebes, plebei.'

Now there is every likelihood that the Latin fifth declension followed the lines of the first. To a Roman of, let us say the third century B.C., the fifth declension was apparently a mere duplicate of the first, with ē instead of ā as its characteristic vowel. *Diēi* was a genitive of the type of *filiāi, familiāi*, while the bye-form *diēs* gen. followed a first declension variety like

familiās; diērum answered to *filiārum, diēbus* to *filiābus*, etc. From the beginning of Roman literature there is a tendency, which gathers additional force in each successive generation, to set ē-forms side by side with ā-forms. To Plautus' *segnities* (cf. *segnitia*), *vastities* (cf. *vastitia*), etc., Terence adds *mollities*, Lucretius *notities, spurcities*, and so on. (For details see Neue's *Formenlehre*.) Often the fifth declension forms oust the others from use, as, for example, the *effigia* of Plautus is superseded by the ē-form *effigies* in classical Latin.

It is precisely this close relation between the first and fifth declension which throws suspicion on a dative like *faciēi* or *fidēi*. For it seems certain that forms like *filiāi, aulāi* were peculiar to the genitive and were never extended to the dative. Priscian, it is true, speaks of disyllabic -ai as a dative as well as a genitive ending, in contrast to diphthongal -ai of the nom., voc. plural (i. p. 291, 17 H.): *nominativus et vocativus pluralis primae declensionis similis est genetivo et dativo singulari. Nam in -ae diphthongum profertur, ut 'hi' et 'o poetae'; sed in his non potest divisio fieri, sicut in illis.* But this statement of his can hardly be correct.

The evidence of the extant literature is all in favour of the disyllabic ending *-āi* being exclusively a genitive, and never under any circumstances a dative ending. And comparative philology, though it has not yet been able to determine with certainty the origin of this curious genitive suffix *-āi*, can nevertheless mark off with exactness the dative termination from the genitive. The dative ending was originally *-āi*, a long diphthong (Gk. *-ā*), which in certain positions in the sentence became *-ā*, a form used for a time in early Latin but afterwards dropped, and in others *-āi*, that is to say the ordinary diphthong *-ai*, which in the classical period became *-ae*. The genitive ending *-āi*, passed (presumably through *-āi*) into the diphthong *-āi* about the time of Plautus, which, like the similar dative ending, became in classical Latin *-ae*. The identity of the gen. and dat. terminations in the classical period is the inevitable result of the phonetic laws of the language. Both reach the same goal, but their starting-point is not the same. In the third century B.C. and earlier genitive *-āi* was quite remote from dative *-āi*.

The fifth declension, we have seen, followed the pattern of the first. We should expect then to find a disyllabic *-ēi*, which through the working of Latin phonetic laws would become *-ēi*, and in rapid utterance even *-ēi* (class. *-i*) in the genitive, but a diphthongal *-ēi* which would become either *ē* or *-ēi* (class. Lat. *-i*) in the dative. That is precisely what we do find in the earliest writer whose works have been preserved in sufficient extent to enable us to determine his habit of speech. Plautus uses *dīēi*, *rēi* (occasionally *rēi*, or with the form of rapid utterance, *rēi*) for the genitive; but makes the dative of *dies* invariably disyllabic, of *res* invariably monosyllabic. His treatment of the *Ē*-stems thus corresponds exactly to his treatment of the *A*-stems. *Rēi* (*rēi*) is with him a genitive, never a dative; precisely as *magnāi* is a genitive, never a dative form: e.g. *Mil.* 103:

magnāi rēi publicāi grātia.

(For details I refer the reader to Seyffert *Studia Plautina* p. 26.)¹ Terence, too, employs no other than a monosyllabic ending for the dative of the fifth declension. The dative of *fides*, for example, is in his plays a

disyllabic word, variously printed by editors as *fidē* and *fidēi*, never trisyllabic. Ennius, Lucilius, and the Republican dramatists, so far as the extant fragments of their writings enable us to judge, followed the same usage.² Laevius (ap. Prisc. i. p. 242 H.) has *quie*.

When we come to the poets of the close of the Republic and the beginning of the Empire, we find great dearth of evidence. The dative singular of a fifth declension word is seldom used. Catullus offers no example of one. But Lucretius, if the MSS. be correct, twice uses the abnormal form *rēi* as dat. of *res*³: i. 687

neque sunt igni simulata neque ulli
Praeterea *rēi* quae corpora mittere possit
Sensibus.

ii. 235

At contra nulli de nulla parte neque ullo
Tempore inane potest vacuum subsistere
rēi.

Horace's *rēi* in *C.* iii. 24, 64:

curtae nescioquid semper abest *rēi*,

is most naturally taken as a dative, though some editors make it a genitive. His *fide* on the other hand is absolutely free from doubt in *S.* i. 3, 95:

prodiderit commissa fide.

We do not get satisfactory evidence of

² Neue (*Formenlehre*² i. p. 378) quotes *fidēi* dat. from Ennius (ap. Non. p. 112 M.). But the manuscripts' reading (see Onions' edition) *reliquae fidei* points to an archetype with *relique fidei*, scanned *rēique fidēi*, and gives no authority for a trisyllabic *fidēi*. His *rēi* dat. in Ter. *Ad.* i. 2, 15 (95) has even less justification. All the MSS. agree in presenting the line in this, the indubitably correct form:

rēi dāre operam, ruri ēsse parcum ac sōbrium.

In the face of all this evidence we can hardly scan the line of Caecilius (*Com.* 25 R.) as: *nil ēgo spēi credo: ōmnis res spissās facit*. Ribbeck scans: *nihil ēgo spēi credo*. The variation however of the MSS. (of Nonius) between *n. rēi ego c.* (H¹GZ¹B) and *n. e. spēi c.* (H²LPVZ²) may point to:

nil spēi ego credo: ōmnis res spissās facit.

The testimony of 'Publ. Syrus' *Append.* 827: *numquam satiss spēi improbae quicquid datur*, is of course of little value.

³ I do not think that any stress should be laid on the fact that *res* was an I.-Eur. *ēy*-stem with dat. sing. *rēy-ai*. For there is every indication that all Latin Fifth Decl. words, whatsoever may have been their I.-Eur. origin, formed their dat. sing. in one and the same way.

¹ Through an unfortunate confusion in the correction of the proofs this fact has been wrongly stated in my *Latin Language*, p. 386, ch. vi. § 25. For 'the same as that of the genitive' read 'the same as that of *Ā*-stems.'

disyllabic *-ei* till we come to Manilius and Seneca,¹ e.g. Manil. iii. 107 :

fideique tenet parentia vincla.

v. 699 :

Nocte sub extrema permittunt jura diei.

Seneca *Thy.* 520 ; obsides fidei accipe. Hos innocentes, frater. *Thy.* 764 : et datas fidei manus. *Phaedr.* 136 neve te dirae spei Praebe obsequentem. The prose authors cannot be used in evidence for the 'disyllabic form of ending. For the older use of the diphthong *ei* was not wholly replaced by the classical spelling *ī* till the beginning of the Empire (e.g. *queis* dat. plur.), so that *fidei*, *diei*, *rei* in Cicero, Caesar, etc., may represent a pronunciation *fidi*, *diī*, *rī*, as well as *fidēi*, *diēi*, *rēi*.² Nor, indeed, can the evidence of our MSS. be wholly accepted even for the spelling *fidei*, *diei*, *rei* against *fide*, *die*, *re*. We know from Aulus Gellius (second century A.D.) how persistently the scribes of the Empire effaced from their copies the antique forms of their originals ; and in one chapter of the *Noctes Atticae* (ix. 14) he mentions two actual examples of the modernising of fifth declension forms (§ 2 corruptos autem quosdam libros repperi, in quibus 'faciei' scriptum est, illo, quod ante scriptum erat, obliterato, and again § 3). Even if a genuine ancient form like *fide*, *die*, *re* did manage to survive the transcription of the Empire, it ran the greatest possible risk of being removed by Carolingian monk-copyists, who in obedience to their text-books of orthography would give every fifth declension dative the ending *-ei*, and would regard a form like *fide*, *die*, *re* in their original as a barbarous misspelling of the same stamp as *paretem* for *parietem*, *quietus* for *quietus*, etc. On the other hand the occasional dative *ī*-forms in our MSS. of Cicero, Caesar, Sallust, Livy, etc., e.g. *republicae* Cic. *Phil.* ix. 1, 2 ; ix. 3, 6 ; xi. 9, 21 (for a fuller list see *Neue Formenl.*² pp. 378-9), deserve to be regarded as survivals of a genuine ancient spelling which was

either frequently or universally employed by these authors themselves.³

More weight attaches to a single statement of Aulus Gellius than to any number of instances that can be quoted from ancient or mediaeval MSS. of spellings like *fidei*, *diei*. This grammarian, who belonged to the second century A.D., discusses in a chapter of his *Noctes Atticae* (ix. 14) the difficulties of the fifth declension, and expressly tells us that the best writers made the dative *facie*, not *faciei* (presumably *faciēi*) : in casu autem dandi, qui purissime locuti sunt, non 'faciei,' uti nunc dicitur, sed 'facie' dixerunt. This is testimony that cannot be set aside ; and it makes the case for *die*, *facie*, *fide* very strong indeed. It is somewhat startling to find how little evidence there is for the familiar forms of our grammars, *faciēi*, *fidēi*, until Silver Age Latin. In the early literature the dative termination is unmistakably monosyllabic ; and this monosyllabic form, whether *-ēi* (class. Lat. *-ī*) or *-ē*, is the only form that is correct according to the phonetic law ; for it is the legitimate development of an original *-ēi*. At some time or other the incorrect form, disyllabic *-ei*, was introduced through false analogy, through analogy apparently of the genitive case. But at what precise time did this spurious form become current ? To answer this question is no easy matter ; and yet it depends on the answer, whether *faciēi*, *fidēi* are to remain in our grammars as the classical Latin forms. I wish that scholars who have made a special study of the text of Lucretius would let us know exactly how much weight they think ought to be attached to the reading of the MSS. in those isolated examples of disyllabic *-ei* in Republican Latin. Even if the reading is above suspicion, it is doubtful how far an imitator of the antique like Lucretius can be taken as an authority for the mode of speech that

¹ Seneca, be it noted, was the first to scan *cui* as a disyllable, *cui*.

² So too in an inscription like the Epist. Praet. ad Tiburtus of c. 100 B.C. (*C.I.L.* i. 201) : neque id nobis neque rei poplicae uostrae otile esse facere, the *ei* of *rei* may express the same sound as the *ei* of *uobis*. FIDE (dat.) on an old inscription of Picenum (*C.I.L.* i. 170) is equally ambiguous, for at this early time E often represents the diphthong *ei*. Cf. SALUTE for *Salutei*, class. *Saluti*, on an inscription of the same period and locality (i. 179).

³ Julius Caesar in the second book of his *De Analogia* declared *die*, *specie*, etc., to be the true genitive forms. We should therefore expect to find these forms in his writings. But the MSS. offer persistently the 'modern' *ei*-forms, though we have *acie* *Bell. Gall.* ii. 23, 1 (for fuller details see Neue, p. 379). This fifth decl. genitive in *-ē*, the existence of which is beyond doubt, seems at first sight to break the natural sequence (1) *-ēi*, (2) *-ī*, (3) *-ēi*, (4) *-ī*. But I think that the true explanation of it is that after the fourth stage had been reached (in the course of the second century B.C.), forms like *progeniī*, *speciī* fell under suspicion of being second declension genitives and were reformed on the analogy of the other cases so as to end in *-ē*, the characteristic vowel of the fifth declension, *progeniē*, *speciē*. The evidence for a dative in *-e* seems stronger than that for a genitive in *-e* in the early literature, e.g. Plautus.

was current in his own day. His *rēi* may quite conceivably be a spurious archaism, like his *supera* for *supra* (cf. SVPRAD on the Sen. Consult. de Bacchanalibus of 186 B.C.)

What has hitherto kept the place of dat. *diēi*, *fidēi* in our Latin grammars free from question has been the belief that the gen. and dat. of the fifth declension were identical forms derived from the same origin; so that

every instance of a disyllabic genitive *-ei* in Latin poetry was taken as evidence for the dative as well as for the genitive. That belief we see to be utterly erroneous, and its rejection involves the rejection of nearly all the evidence for a disyllabic *-ei* in the dative of the fifth declension in classical Latin.

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NOTES.

PLAUTUS, *Amphitruo*, 343 (Goetz-Schoell):

ME. Servosne <es>an liber? So. Ut quomque animo conlubitumst meo.

ME. Ain vero? So. Aio enim vero. ME. Verbero. So. Mentiris nunc.

The last two words in this passage are evidently the most important of all. Prof. Palmer thinks that Sosia speaks thus 'because Mercury had said *verbero* (scoundrel), which Sosia pretends to understand as the present indicative.' I have never been able to accept this explanation. Mercury, surprised by Sosia's pert answer, *Ut . . . meo*, ejaculates, *Ain vero?* 'What's that you say?' the tone giving his utterance a force like 'Can I have heard aright?' Sosia, punning, pretends to take Mercury's question literally as meaning 'Do you speak the truth?' and hence answers by saying 'Why, yes, of course I do.' Mercury, disgusted by the pun, cries *Verbero* 'Wretch,' to which Sosia replies, 'That's a lie you're uttering now.' He is not a *Verbero*, but Sosia and a *servus*.

Two points call for notice. (1) *Ain vero?* For the force given above to these words see Langen, *Beitr. zur Krit. u. Erklär. des Pl.*, p. 119: '*ain, ain tu, ain vero, ain tandem* stehen entweder als Unterbrechung der Rede eines Andern beim Beginne der Gegenrede oder als blosse Unterbrechung zum Ausdruck der Verwunderung, Ueerraschung über das, was ein Anderer gesagt hat, im Ganzen bei Plautus mindestens fünfundzwanzigmal Mal . . .' That it was perfectly possible, however, for Sosia to interpret them literally, without doing the least violence to the language, as meaning 'Are you telling me the truth?' can be seen (a) from such a passage as *Pl. Epid.* 699 where *Ain tu? lubuit?* is answered by *Aio, vel da pignus, ni east filia*, and (b) from the

fact that not infrequently in Plautus *vero* = 'truthfully,' 'truly.' See Brix-Niemeyer on *Captivi*, 567.

(2) If we had *Mentiris tu nunc* in the text instead of the simple *mentiris nunc*, every one would, I think, admit at once that the interpretation advanced in this note would be inevitable. I do not believe, however, that it is really invalidated by the absence of *tu*. I would reason thus. The primary contrast in the passage is not between the persons: hence we have neither *ego* with *aio enim vero* nor *tu* with *mentiris nunc*. The real antithesis is rather between the actions, between *aio* and *mentiris*.¹ This opposition has been emphasized by placing the verbs first in their respective clauses. Further, the actor could without difficulty make this contrast clear. Finally, since *mentiris* carries its own subject with it, the emphasis placed upon it brings out sufficiently the secondary contrast between the persons.

Plautus, *Captivi*, 769 ff.

Maxumas opimitates opiparasque offers mihi:

Laudem, lucrum, ludum, iocum, festivitatem, ferias,

Pompam, penum, potationes, saturitatem, gaudium.

The note on *pompam* in the Brix-Niemeyer edition runs as follows: '*pompa*, vgl. Plaut. fragm. Baccar. *quoniam haec ventri portatur pompa*?'² von einem massenhaften Marktein-

¹ I have tried to bring this out by translating above 'That's a lie you're uttering now!'

² This is the only passage cited by Lewis and Short for this meaning of *pompa*, but the reference is wrongly given as Macrob. *Sat.* ii. 12. Correct to iii. 16. 1, (Eyssenhardt). The same error is made by Friedlaender in his note on Petronius 60, to be cited presently. Brix-Niemeyer, though they had this place in mind, do not give the exact reference.

kauf für die Küche, der beim Nachhause-tragen das Bild eines Prozessionsaufzuges bot. Stich. 683 *agite, ite foras: ferte pompam.* Hallidie says rather vaguely: 'In the Latin dramatists it (pompa) is used of provisions and other requisites for a banquet.' One cannot help regretting that no citations are given in support of this statement. If we may trust Ribbeck's indices, the word does not occur at all in the fragments of the tragic or the comic writers. I feel sure that it does not occur, at least in this sense, in Terence. Gray makes no comment on Stichus, l. 1.

Note that in the *Baccaria*, as in the *Captivi*, *pompa* is used in this peculiar sense by a parasite. In the Stichus the speaker is a slave. We may, perhaps, conclude that this use is in its origin colloquial and plebeian.

I have noted another good parallel in Petronius, 60: *Iam illic (= in mensa) repositorium cum placentis aliquot erat positum, quod medium Priapus a pistore factus tenebat, gremioque satis amplo omnis generis poma et uvas sustinebat more vulgato. Avidius ad pompam manus porrexit.* . . . Friedländer cites Martial x. 31. 3, 4

Nec bene cenasti: nullus tibi quattuor emptus

Librarum cenae pompa caputque fuit,

and xii. 62, 9

*Cernis ut Ausonio similis tibi pompa macello
Pendeat et quantus luxurietur honos?*

Only one of the three passages in Plautus, that from the *Baccaria*, is cited by him, but, as noted above, the reference is wrongly given.

Plautus, *Trinummus*, 533-537 (Brix-Niemeyer):

Neque umquam quisquamst, quouis ille ager fuit,

Quin pessume ei res vorterit. Quorum fuit, Alii exolatum abierunt, alii emortui, Alii se suspendere. Em, nunc hic quouis est Ut ad incitast redactus.

With the description of the ill luck attending the owner of this field compare what Aulus Gellius iii. 9 says of the *equus Seianus*. Especially interesting is § 3 *eundem equum tali fuisse fato sive fortuna ferunt, ut quisque haberet eum possideretque, ut is cum omni domo, familia fortunis*

que omnibus suis ad interneccionem deperiret. In §§ 4 and 5 follows a list of the calamities that befell the successive owners of the horse, and in § 6 we read: *Hinc proverbium de hominibus calamitosis ortum dicique solitum: Ille homo habet equum Seianum.*

In § 7 Gellius quotes another proverbial expression for an unfortunate possession, *aurum Tolosanum*, adding: *Nam cum oppidum Tolosanum in terra Gallica Quintus Caepio consul diripisset multumque auri in eius oppidi templis fuisset, quisquis ex ea direptione aurum attigit misero cruciabilique exitu periit.*

Terence, *Phormio*, 140 ff.:

GE. *Ad precatorem adeam credo, qui mihi
Sic oret: 'nunc amitte quaeso hunc;
ceterum
Posthac si quicquam, nil precor.' Tan-
tum modo
Non addit: 'ubi ego hinc abiero, vel
occidito.'*

Add to Dziatzko's note a reference to Plaut. *Epid.* 687 (Goetz-Schoell), which contains an allusion to the *precator*. Cf. also Petronius, 49: *Nondum efflaverat omnia, cum repositorium cum sue ingenti mensam occupavit. . . deinde magis magisque Trimalchio intuens eum, 'Quid? quid?' inquit, 'porcus hic non est exinteratus? Voca, voca cocum in medium.' Cum constitisset ad mensam cocus tristis et diceret se oblitum esse exinterare, 'Quid? oblitus?' Trimalchio exclamat, 'putes illum piper et cuminum non coniecisse. Despolia.' Non fit mora: despoliatur cocus atque inter duos tortores maestus consistit. *Deprecari tamen omnes coeperunt et dicere: 'solet fieri; rogamus, mittas; postea si fecerit, nemo nostrum pro illo rogabit.'**

Horace, *Satires*, i. 1. 49:

*vel dic quid referat intra
naturae finis viventi, iugera centum an
mille aret?*

So far as I have noted, Kiessling alone of recent editors comments on the function of *vel*. His statement is: '*vel* verknüpft nicht *dic*, sondern die Frage *quid referat*, als einen neuen Versuch die Unvernunft des ewigen Zusammenhäufens darzuthun, mit *non tuus capiet venter plus ac meus*' in v. 45. This statement seems to me in part erroneous. Does not *vel* rather join the question *quid referat* to the question already put in v. 44: *quid habet pulchri constructus acervus?* In

this way we make *vel* connect the two attempts thus far made 'die Unvernunft des ewigen Zusammenhäufens darzuthun.' It is hardly necessary to quote examples in support of the connection of questions by adversative conjunctions. A simple reference to passages like Verg. *Aen.* i. 369 *Sed vos qui tandem, quibus aut venistis ab oris, Quove tenetis inter*, or Livy i. 1. 7 *percunctatum deinde, qui mortales essent, unde aut quo casu profecti domo quidve quaerentes in agrum Laurentem exissent*, and Weissenborn's note, will suffice.

Horace, *Satires*, i. i. 68 ff.:

Tantalus a labris sitiens fugientia captat
Flumina—quid rides? mutato nomine de te
Fabula narratur: *congestis undique saccis*
Indormis inhians, et tamquam parcere sacris
Cogeris aut pictis tamquam gaudere tabellis.

Of recent editors some—Palmer, Wickham, Greenough—are silent about the words italicized in the foregoing passage, others—Schütz, Kiessling, Orelli-Mewes (editio maior), Kirkland—agree in taking *undique* as = 'from every side.' Schütz writes on v. 71: 'Der Geizhals schläft auf seinen *undique* (*per fas et nefas*) zusammengerafften Geldsäcken mit aufgesperstem Munde, d. h. gierig nach mehr, wie der durstige Tantalus nach Wasser; selbst im Schläfe verlässt ihn die Begierde nicht.' Cf. Kiessling: 'Nicht die *sacci* sind *undique congesti*, sondern das in ihnen enthaltene Geld; aber *saccis* ist um des Wortspiels mit dem folgenden an derselben Versstelle *sacris* gewählt;' Orelli-Mewes: '*congestis undique* "quos omni quaestus genere parasti;"' Kirkland, 'gathered together from every side; i.e. by every means of gain.'

I prefer to take *undique* here as = 'on every side.' If we so interpret, we shall not need to take *saccis* as put by metonymy for the contents of the bags rather than the bags themselves, and we shall, I think, get a closer parallel between the miser's situation and that of Tantalus. The miser, falling asleep, with mouth agape, in the midst of the money bags piled high on every side of him might well remind one of Tantalus with mouth open trying to catch the abounding waters that touch his very lips. If I may use the phrase, the miser is in the midst of a flood of money bags, even as Tantalus is in the midst of the flood of waters. Note too that with this view *congestis* at once receives additional point as suggesting the same idea of abundance in the miser's case that *flumina*, v. 69, does in the case of Tantalus. We have a second pair of related pictures in the balancing words *captat* and *inhians*: see Kiessling ad loc. The one by implication pictures Tantalus' open mouth as he seeks to drink, the other by direct statement brings vividly to the mind the figure of the miser greedily gaping over his gold.

Horace, *Satires*, i. 5. 50:

Hinc nos Coccei recipit plenissima villa

For a good commentary on *plenissima villa* cf. Cicero, *Cato Maior*, § 56: *Semper enim boni assidue domini referta cella vinaria, olearia, etiam penaria est, villaque tota locuples est, abundat porco, haedo, agno, gallina, lacte, caseo, melle. Iam horum ipsi agricolae succidia alteram appellant.*

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LATIN BARBA AND ITS INITIAL B.

"How is Lat. *barba* 'beard,' for which we should expect **farba*" from Idg. **bhardhā*?¹ "(O.H.G. *bart*, O. Bulg. *brada*) to be explained? All the attempted explanations known to us are unsatisfactory."

So wrote Professor Karl Brugmann in 1886 (*Grundriss*, vol. i. § 338 Rem.), and still in 1896 the question awaits an answer.

Professor F. Stolz, *Lat. Gr.*², 1890, § 55,

¹ See Brugmann, *Grundr.* vol. i., §§ 338, 370; and Stolz, *Lat. Gr.*³, 1890, § 55, p. 295.

p. 295, and, more recently again, Mr. W. M. Lindsay, *The Lat. Lang.*, 1894, ch. iv. § 104, p. 283, have sought to explain the initial *b* of *barba* as due to assimilation. But were that so, why have not *faber*² and *fiber*³ likewise become **baber* and **biber*?

Before I venture to offer what I believe

² Containing *f* from Idg. *dh*, see Brugmann op. cit. i. § 379; and Stolz. *l.c.*

³ Containing *f* from Idg. *bh*, see Brugmann op. cit. i. § 341.

to be a hitherto unsuggested solution of this difficult problem, it will be of use to consider the evidence at hand relative to the date of the initial *b* of *barba* :—

That an eminently early date must necessarily be assigned for the supersession of the initial *f* of Lat. **farba* by *b* is proved by the Latin name *Barbatus* found twice on the inscriptions on the tombs of the Scipios :— (1) on that of L. Cornelius Scipio Barbatus the consul of 298 B.C. (*C.I.L.* i. 30, an inscription which according to Ritschl "dates not later than 520 U.C.," 234 B.C.), and (2) on that of Barbatus' son, L. Cornelius Scipio, the consul of 259 B.C. (*C.I.L.* i. 32, which, according to Ritschl, was "probably written about 500 U.C." 254 B.C.).¹

Having thus shown the antiquity of the initial *b* of Lat. *barba* (for which **farba* was to have been expected as coming from Idg. **bhardhā*), I may venture to offer my own explanation. I would suggest that Lat. *barba* owed its initial *b* to the influence of Celtic,² influence which may be dated either 390 B.C., the year of the invasion and occupation of Rome by the Celts under Brennus,³ or indeed at any time in the first half of the fourth century B.C., during which the Gauls (as the Romans called them⁴) or Celts (as they called themselves⁴) "often returned to Latium" (Mommson, *The History of the Roman Republic*, abr. ed., 1891, ch. ix. pp. 80, 81).⁵

¹ Of the Latin proper-names formed on *barba* (e.g. *Barba*, *Barbo*, *Barbatus*, *Barbula*, *Ahenobarbus*) *Barbatus* is, I think, the only one occurring on inscriptions so early as the 'Inscriptiones vetustissimae bello Hannibalo quae videntur anteriores' (= *C.I.L.* i. Pars Prior), and therefore the only one which is of use in the present enquiry.

² For *b* was the regular representative of Idg. *bh* in Prim. Celtid; compare e.g. O.Ir. *bri* gen. *breg* 'mountain,' Gall. *brigi* (in *Brigantia*, *Are-brigium*): Skr. *brh-ant* 'great, high,' Armen. *barjr* 'high,' root-form **bhrgh-* (see Brugmann, *op. cit.* vol. i. § 341).

³ 'Brennus,' or, to be strictly correct, 'The Brennus' (*Brennus* merely signifying 'king' or 'chief').

⁴ See F. Max Müller, *Lectures on the Science of Language* (new edition 1882), vol. i. Lect. v., p. 225 note.

⁵ If not too fanciful, an argument in favour of the earlier date, 390 B.C. may be drawn from the familiar legend that it was to the *stroking of the beard of M. Papirius* by one of the invading Celts and the consequent retaliation wherewith the latter sought

The Celts themselves are known to have borrowed the Latin word for 'gold,' namely *aurum*⁶ (whence Irish *ór*, Cymr. *awr*, Camb. *our*, *eur*). Hence it is not an extravagant presumption that they in their turn may have left on Latin some traces (however slight) of their own language.

It well deserves mention here that there is good ground for believing Lat. *gladius* 'sword' to have been in reality a loan-word from Celtic: Welsh *clodd* *clddyf* 'sword' (cf. e.g. St. Matthew xxvi. 52, *Dychwel dy gledddyf i' w le: canys pawl a'r a gymmerant gledddyf*, a *ddifethir â chledddyf*, "Put up again thy sword into its place: for all they that take the sword, shall perish with the sword"), Gael. and Ir. *claidheamh* 'sword' (seen also in *claidheamhmòr* 'a great sword, broadsword,' more recognisable in the anglicised spellings *claymore* *glaymore*).⁸

On the evidence at our disposal (meagre though it is admitted to be) I venture to believe that we may be right in regarding the initial *b* of Lat. *barba* (beside the correct Lat. **farba*) as one of the traces of Celtic influence on Latin.⁹

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to avenge the insult, that the general slaughter of the aged Roman senators who had refused to leave their ancestral halls was due (see T. Arnold *History of Rome*, vol. i. ch. xxiv. pp. 543-545).

⁶ Latin *aurum* was borrowed by the Celts from Latin [after the date of 'rhotacism,' concerning which see the second of 'Two Papers on the Oscan Word *Anasaket*' (London: D. Nutt, nearly ready) § 7, note] at "the time of the great Celtic movement southwards . . . which introduced the black day of Allia (390 B.C.) into the Roman Calendar." See O. Schrader, *Prehistoric Antiquities of the Aryan Peoples*, Engl. ed. 1890, Part iii. ch. iv. p. 177.

⁷ This possibility has already been noticed by King and Cookson, *Sounds and Inflections in Greek and Latin*, 1888, p. 126).

⁸ The word *gladius* occurs in the *Annales* of Ennius (239-169 B.C.), while the diminutive *Gladiolus* (*ἑρξελπίδιον*) is found as the title of a comedy of Livius Andronicus (flor. 240 B.C.). It is thus obvious that the word (if a loan-word) must have been borrowed at a fairly early date.

⁹ At what date was the word *barbarus* borrowed by Latin from Greek? (For the fact of its having been so borrowed see Stolz, *Lat. Gr.* § 42, p. 283). Was it borrowed so early as the first half of the fourth century B.C.? And, if so, could the transition of Lat. **farba* to *barba* under the influence of Celtic possibly have been aided by the common use of the word *barbarus* among the Romans as an appellation of the Celts?

NOTES ON VIRGIL, *GEORG.* II. 501-2.

THE explanation of *populi tabularia* quoted by Mr. Ray from Forbiger in the October number of the *Classical Review* rests on a more respectable authority than Forbiger's, being taken verbally by Forbiger from Heyne's commentary. But Mr. Ray does not seem to have noticed that it bears a different sense from that which he attaches to it, and that the sense which he attaches to it does not suit the drift of the passage. 'Happy is the peasant,' Mr. Ray explains the phrase, 'who has not seen the grinding injustice of the tax-farmers.' It is obvious to remark that this was precisely what the peasant did see, and the inhabitant of Rome did not. But Heyne's explanation is something quite different: 'Happy is the peasant who has not dealt in public contracts.' His simple and natural life is contrasted with that of the financier, as, in the words which immediately precede, it was contrasted with that of the lawyer and politician.

This explanation gives Virgil's phrase a rational and appropriate meaning. But whether the words will bear it is a different question. These contracts of the *publicani* were only one sub-division of the mass of public records preserved in the Roman Record Office attached to the Temple of Saturn, and not even the most important sub-division. One of the two notes on the phrase in the Servian commentary is in the following apt and accurate words: '*negotia publica et rationes populi, quae in tabulis scribuntur, unde tabularia dicta.*' So far as the phrase expresses an abstract idea it can hardly be restricted to any more special meaning.

But what is important to grasp—as the *Georgics* are a poem and not a treatise on political economy—is not so much the abstract idea in Virgil's mind as its imaginative embodiment. The mere use of the word *vidit* rather than *novit* indicates that urban life rises inevitably before the poet's mind

in a concrete shape. This imaginative instinct, which must needs think in visible forms, acts in the moulding of Virgil's sentence with accumulating force. The first touch of concrete form is given by the epithet, *ferrea*, attached to the abstract word *iura*. In the next member of the sentence the process goes a step further, and political life is now presented under the visible and tangible symbol of the Roman Forum, the central spot of its action. But at this point the imagination has gathered so much momentum that it will not stop. 'The mad forum,' the flat paved space filled with its seething crowd, is actually present to Virgil's inner eyes; and as part of the same picture, the vast mass of the great Record Office across its upper end, a silent background to the shouting orators and surging mob below.

I follow Mr. Ray in using the word Record Office. But that particular *tabularium* was more than this. For a proper English parallel we must conceive of the Treasury and the Record Office in one building occupying the site of the National Gallery; with Westminster Abbey close behind and above them, the Houses of Parliament and the Law Courts sitting in the Royal College of Physicians or St. Martin's Church, and the general elections for the whole country, speeches and all, going on in Trafalgar Square.

There could hardly be a more complete instance of the organic imagination caught, if one may say so, at work. Curiously enough Ovid, so often an unconscious commentator on Virgil's methods, unites the first and last steps of the Virgilian climax, in a line describing the House of the Fates (*Metam.* xv. 810), *solido rerum tabularia ferro*. The contrast could not be more neatly put between the creative and the mechanical imagination.

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NOTE ON HOMER HYMN DEM. 268.

WHEN I see the most brilliant of our younger Homeric scholars proposing ἀθανάτοις θνητοῖσι τ' ὄνειρα καὶ πολὺ χάσμα, another of the established reputation of Prof. Tyrrell suggesting ὄνειρα κάππα τέρεται, and

finally Mr. Allen raising no objection to the latter on metrical grounds,¹ I cannot refrain from pointing out that a syllable naturally short cannot be lengthened at the

¹ *Classical Review* for last month, pp. 388, 393.

end of the fourth foot by position,¹ unless it forms a monosyllabic word. There are no doubt a few exceptions in Homer but so few and so doubtful that they afford no support for importing another. Thus in the phrase βῶπις or βῶπι πότνια Ἥρη the ι is certainly long and we should perhaps accent βῶπις ποτνία. At Φ 126 we should read, I now think, μελαίνῃ φρήνι(ι) ὑπαίξει.

¹ Unless of course the consonant or consonants lengthening it are part of the same word.

The rule was observed throughout the whole course of Greek epic verse. Indeed in the late highly polished school of hexameter writers it is still more stringent, for they decline to lengthen even a monosyllable in *thesi* at this part of the verse, at any rate generally speaking.

I believe that the only two exceptions to the Homeric rule in the Hymns are xxxii. 6 and xxxiv. 18, a precious pair of lines.

ARTHUR PLATT.

THE NEW EDITION OF BUSOLT'S *GRIECHISCHE GESCHICHTE*.

G. BUSOLT: *Griechische Geschichte*, Band ii. *Die aeltere attische Geschichte und die Perserkriege*. Zweite vermehrte u. völlig umgearbeitete Auflage. 1895. Large 8vo. pp. xviii. 814. 13 Mk.

The second edition of Professor Busolt's chief work is all, and more than all, it professes to be, an enlarged and thoroughly revised version of the first (1885-1888). It is virtually a new and in every way a bigger book. It is also a better book, an observation not necessarily consequent on the preceding. The improvement arises not so much from any change in the author's method, as from the notable additions to our resources which have been made during the last ten years, since the first and second 'Parts,' which have now grown into the first and second 'Volumes' of this *History*, saw the light. From two different quarters Greek history has received large endowments, by the Mykenaeen renaissance—it must still for convenience be called Mykenaeen—and by the discovery of the Aristotelian *Polity of Athens*. These original additions have naturally been attended by a huge and rapid output of treatises and articles, a formidable increment in the bibliography of our subject. It is enough to make less capable or more distracted students well nigh despair to see with what apparent ease Professor Busolt not merely utilises the additions to our original sources, but also digests the masses of accumulating exegesis, down to the last German monograph, before going to press. His exemplary diligence in this respect would make his work indispensable to all students of Greek history, quite apart from the value of his own contribution to the discussion raised by the new material, and by the literature

arising out of it. This growth of materials has led the author not merely to enlarge his volumes, but to re-distribute his chapters and paragraphs, and, indeed, to renumber and to rename them. The effect here is all for the better, and fully bears out the author's prefatory claim to exhibit a more thorough-going analysis of the original sources, and a more convenient synthesis of results than in the first edition. Yet, here I venture to suggest *argumenti causa* that the new first chapter (*Die mykenische epoche* i.² 3-126), useful and interesting as it is in itself, somewhat disturbs the symmetry and even the method observable in the *Handbuch* as a whole. This chapter is in the first place an inventory and description of the material remains of the so-called Mykenaeen period. It is in the second place a survey of the geographical distribution of those remains, and a discussion of the antecedents and origin of the Mykenaeen culture, with results probably not all acceptable, even now, to our leading archaeologists. It is not, and indeed it could not be, a history of the Mykenaeen period; the time is not yet come for that. This first chapter is preceded by three pages on the sources and recent bibliography (*Quellen und Literatur*): but the description of the archaeological evidences is here the description of the real *Quellen*, the most authentic, the most primitive. The second chapter deals with the origin of the historical complex of Greek states (*Die Entstehung der geschichtlichen Staatenwelt*): but the 'Mykenaeen' states are becoming more real than some of their successors: they had their constitutions, their cults, their economy, their politics, as well as their arts, and arms, all which can hardly be relegated permanently to the 'præhistorie' limbo. In truth, Busolt's

present arrangement can be but transitional. We may hope to see in the third, or in the fourth edition of the same work from the same learned pen, a further stage reached in the thorough-going analysis of evidences, and in the convenient synthesis of results. Meanwhile the book in its present form may safely be taken to exhibit more fully and fairly than any similar work the position of the whole argument down to the date of its publication (1893): and we can trust the indefatigable author, when the time comes for a retraction of the problems discussed in his first and second chapters, to place his readers once again fully abreast of the ever-growing argument.

The large amount of space devoted in the first volume to the Mykenaeen question, and the discovery meanwhile of the *Ἀθηναίων πολιτεία* entailed the transfer of *Early Attic history* to the second volume, with which indeed we are here more directly concerned: nor merely the transfer, but a wholesale reconsideration, only some few degrees less far-reaching and novel than the results of that Mykenaeen renaissance before recorded. In dealing with the new text, a source, or at least a 'channel,' (to borrow a distinction from v. Wilamowitz-Moellendorff), of a class with which a scholarly historian is of course well qualified to deal, Professor Busolt naturally moves with even more authority than among the ruins and relics of Mykenai. We have all tried our hands, with more or less success, on the text, or on the contents, of the recovered treatise: and for a while the English contribution to the new debate was both prompt and ample. It must now be confessed that with the works of Kaibel and of Blass, of Wilamowitz and of Busolt before us—to name only the more considerable representatives—Germany is leaving us behind. It was bound to be so. What chance has a lecturer in Oxford—or, for aught I know, in Dublin or in Cambridge—of getting an audience together, out of our 'Mods.' ridden, 'Greats' ridden, Tripos ridden, Civil Service haunted first-classes, to stand such a course of deliberate and exemplary analysis, as we see deposited in *Aristoteles und Athen*, even assuming the genius and learning among us to essay it? There may be better times in store for those now condemned, or permitted, 'to bow themselves in the house of Rimmon'—our examinations-idol—but meanwhile his votaries are fain for the most part to serve this false god with dainties condensed from the works of those German prophets. But to return:—it is no matter for regret that

Professor Busolt had printed the first two hundred pages of his second volume before the appearance of *Aristoteles und Athen*, and has only been able to use that brilliant and suggestive work for the history from Drakon onwards, and, for that, only after having worked out his own results. We have thus in the volume before us, and especially in the forty pages devoted to the discussion of the new authority on its own merits, a more independent, or perhaps a less polemical, appreciation of the *Ἀθηναίων πολιτεία* than is possible to any one now, at least until he has accepted, or refuted, von Wilamowitz-Moellendorff. Thus it will count for something with those, who may not be able to form an independent judgment, that Busolt, like v. Wilamowitz, regards the *Polity* as Aristotle's. It would save a deal of trouble, no doubt, even in the matter of mere citation, to be convinced that we might quote the treatise as Aristotle's, *sans phrase*. But even the ingenious manner in which v. Wilamowitz dovetails the composition of the *Politics*—or of the various courses of lectures which that work may represent—into the composition of the *Polity*, is rather suggestive than convincing. Perhaps those who doubt the strict Aristotelian authorship of the *Polity* may have been expecting too much from the historical excursions of the father of Logic: but 'very Aristotle' will still seem to many an hypothesis unnecessary to explain any of the data, and well-nigh irreconcilable with some of them. Apart from the traditional ascription of the *Ἀθηναίων πολιτεία* (plus 157 similar tracts) to Aristotle, would any scholar have identified the author of the Br. Mus. Papyrus cxxxi. with the author of the *Politics*? Well, yes, perhaps the brilliant writer of *Aristoteles und Athen*, who has convinced himself, and Professor Busolt too, that the author of the oligarchic party-pamphlet, which we all recognize among the sources of the *Polity*, was Theramenes, son of Hagnon, of Steiria, and none other. This identification adds not merely a fresh fame to Theramenes, but a new name to the list of Greek authors: for, it was not previously proved that Theramenes had published anything, no, not even his own speeches (*A. u. A. i.* 167). But it was, we are now told, from this lost and forgotten work of Theramenes that Aristotle derived, at the eleventh hour, after writing the well-known passage in the *Politics* on Drakon (2, 12, 1274b), that later account of the Drakontic Constitution, which formed one of the surprises of the new-found *Ἀθηναίων*

πολιτεία. Busolt may have done well in cancelling his acceptance of the hypothesis that Kritias indited the said brochure: but the tempting ascription to Theramenes is unprovable. If an authentic work by Theramenes had just come into 'Aristotle's' hands, and he was borrowing largely from it things new and old, it is a little unfortunate that no reference, however remote, to the literary activity of Theramenes occurs in the text. The praise of him by name in association with Nikias and Thukydides [son of Melesias] makes nothing for his authority as a writer, but rather the reverse, especially as it occurs in a context, for which Theramenes cannot have been 'Aristotle's' authority. It is one thing to suppose that the writer of the *Polity* had a more or less authentic report of a speech, or of speeches, of Theramenes in 404 B.C., or in 412-1 B.C., and used them in writing his accounts of the Revolution of the Four Hundred and of the Régime of the Thirty; it is another thing to name Theramenes as author of a never-cited tract, in which the Drakontic Constitution was set out, with much more to the same effect. But even if the description of the Drakontic Constitution in 'Aθ. πολ. 4' were demonstrably traced to the pen of Theramenes, that would leave its historical character as dubious, nay, as discreditable, as ever. Busolt has not been beguiled into accepting von Wilamowitz-Moellendorf's verdict on the historical value of this passage, and Busolt's opinion on this matter is the more valuable, as he was originally prepared to reconstruct the constitutional history of Athens, upon the supposition that the Drakontic Constitution was a distinct and authentic stage in the order of events. The argument of *Aristoteles und Athen* helps to vindicate the passage as a genuine part of the original text, and plausibly nominates an ultimate authority for this novel and inconsequent chapter in Athenian history; but it has done very little (in my opinion) to render the passage acceptable as a real addition to our knowledge of the state of Prae-Solonian Athens, and for this conclusion it is pleasant to be able to cite the authority of Busolt's second thoughts.¹

¹ To avoid misunderstanding, it may be well to note that the sceptic is not bound to deny the restriction of the franchise, in Drakon's time, to the citizens who carried arms, and themselves provided the arms they carried (οἱ τὰ ὅπλα παρεχόμενοι). But the crucial question is whether Drakon was the author of a new Constitution, and of a new Constitution which is fairly described in 'Aθ. πολ. 4'. Is the authority or the argument of the *Polity* enough to carry that conclusion? I trow not.

Concerning still more primitive times and institutions, and their treatment by Busolt, and other German authorities, I can but allow myself here one general remark. Undoubtedly something may be recovered in regard to the character of 'ancient law,' from texts and inscriptions of the fourth and subsequent centuries, and a historian is bound in the first instance to make the most of the direct evidences, so far as they go. It seems, however, a shortcoming that recent investigation in Germany ignores, or even deliberately rejects, the assistance of analogies furnished by the comparative and anthropological methods to Hellenic *origines*. The primitive, or relatively primitive, condition of society, of government, of domestic and religious institutions within the area of later Hellenism will never be fully understood, without recourse to anthropology. We have the best precedent for the position, for there is hardly a method employed by anthropology to day which is not potentially used by Thucydides in his immortal poem, on the beginnings of Greek history. From this point of view an English reader may be struck by the amount in Busolt's section on the *Beginnings of the Athenian State* (§ 15), which is valid or verifiable only for post Eukleidean Athens. Aristotle, in the *Politics*, fell into the mistake of supposing that the analysis of the domestic institutions of Athens in the fourth century supplied the clue to the historic origin of the city-state. He formulated the parent idea which Sir Henry Maine, twenty-two centuries later, represented as 'The Patriarchal theory.' The name may be a mis-nomer, but we cannot get rid of it now, the rather, as it has provoked the not less objectionable term 'matriarchate,' to describe that condition of society, in which kinship is traced through females chiefly or exclusively, and institutions conform, in a greater or less degree, to this uncivilised precedent. For German Hellenists I will not say the works of McLennan, but the works of their own savant, A. H. Post, apparently do not exist. (Those writers are both gathered to their fathers, and can be named without fear or favour). To take one instance; the importance of the *Avunculate*, or mother's brother's right, in early Hellenic, or apparently Hellenic, society, is hardly to be explained save by analogies, of which anthropologists can supply any number. Some curious points in Athenian law, e.g. the legality, under certain circumstances, of marriage between children of one father, may be in part explicable as survivals of

'matriarchal' rights. Again, no one acquainted with the literature of this subject is likely to acquiesce (with Busolt, p. 114) in the interpretation of *δμογάλακτες* (Milk-brothers?) as originally 'the descendants of a common Father.' We shall never get to the bottom of the problems touching the nature and origin of tribes and phratries, or understand the revolution, or evolution, which passed over society in Attica and elsewhere, in the beginnings of history, by the mere analysis and description of society as it was in the fourth, or even in the fifth and sixth centuries, within the strictly Hellenic city-states.

It is not possible here to discuss the mass of details upon which issue might be taken with the learned author of this large yet closely packed volume, but I may note a few of the points specially interesting to myself. (1) Busolt rejects Beloch's suggestion that the stories of the two expulsions of Peisistratos are duplicates in disguise; but I do not find his refutation (p. 320) quite conclusive. On this point v. Wilamowitz agrees with Busolt: but v. Wilamowitz himself detects a *doublette* in the Herodotean stories of the Atheno-Aiginetan wars, and it is doubtful if the greater chronological consistency of the Peisistratid tradition, can rescue the stories in detail. (2) Busolt (pp. 167, 583) retains the view that at Athens in 490 B.C. the supreme command circulated day by day within the strategic college. I have elsewhere (I trust) made it more probable that at Marathon the Strategi were still Colonels of the phyleic regiments, and the 'War-Lord' still in supreme command. (3) Busolt (p. 528), accepts the story of the conduct of Miltiades at the Danube, the incredibility of which Thirlwall long ago pointed out, and the origin of which I have elsewhere tried to explain. (4) Busolt retains his former chronology for the Ionian revolt, by which the siege of Miletus is made out to have lasted three years: objections and alternatives to this chronology I have urged elsewhere at sufficient length.

It is natural that in undertaking to digest not merely all the ancient authorities but nearly all the immense literature of the present day upon our subject, Professor Busolt should now and then make himself responsible for discrepant utterances. Thus (on p. 650) the anecdote about Themistokles and the increase of the Athenian fleet told in the *Ἀθ. πολ.* is dismissed as 'highly improbable in itself, and a contradiction of the older sources,' while on the next page an element of truth is conceded to it. If

Prof. Busolt had happened to recall, in this connexion, the statement of Herodotus concerning Kleinias son of Alkibiades and his own trireme (Hdt. 8, 17), he might have found the contradiction less absolute, and the element of truth somewhat more probable. In dealing with the stories of the Persian wars, which form the second theme of this volume, the author could not exhibit such an advance on the previous edition of his work as in the earlier chapters, for there has been little fresh evidence to consider. His duty has been of necessity confined to a report on the ever growing bibliography, and a revision of his own previous positions in view of more recent discussions. It is to be regretted that the author cannot have seen Mr. G. B. Grundy's map of the battle field of Plataea, with accompanying paper, published by the R. G. S. in 1894, as that sound bit of work has completely antiquated previous surveys. Among recent studies H. Delbrück's brilliant monograph appears to have exercised some influence on Busolt's treatment of the Persian wars, and he has gone the length of accepting the *Visions-hypothese* as the true explanation of the celebrated Shield-episode at Marathon: but he reacts freely, as might be expected, against the exaggerated scepticism of H. Welzhofer, who is a veritable *advocatus diaboli* in regard to the canonisation of Herodotus.

This second edition does not reach the point at which the first edition ended: the history of the *Pentekontaetia* is relegated to the third volume, for which probably we shall not now have long to wait. Whether that third volume will carry us down to the end of the fourth century remains to be seen: but those who know the *Forschungen zur griechischen Geschichte* (1880), and remember that Dr. Georg Busolt made his début with a substantial monograph on 'The Second Athenian League' (1874), are looking forward with the liveliest interest to the remaining volumes of this *History*. On the scale now ruling the work the third volume, which was originally intended to reach the battle of Chaironeia, can scarcely go lower than the archonship of Eukleides: but it may be hoped that a fourth and final volume will appear before a new edition of the earlier volumes is demanded. This hope may look rather like a left-handed compliment, but it is expressed in the interests of the author and of his subject. The later volumes will fill a gap left by the abrupt close of Duncker's great *History*. Busolt's work is dedicated to Duncker, that is now

to his memory. It is becoming the fashion in some quarters to dismiss Duncker as the modern Ephoros, who bedizened the native simplicity of the historic Muse with his rationalism and his rhetoric: but whatever may have been the value of the Egyptian and Oriental portions of his work, in regard to which Duncker could not himself control the native sources, his contribution to the discussion of the problems of Greek history is not to be despised, and its sudden cesser with the second year of the Peloponnesian war was a real misfortune. That misfortune Busolt's forthcoming volumes will more than compensate, but the loss will not be in every respect covered. Busolt's work

is a monument of learning, and of scientific exposition: he has deliberately sacrificed upon that altar the charms of literary art. His work is conscientiously devoid of rhetorical merit, and it is no mere pastime to read it from cover to cover. Very full tables of contents facilitate the use of the volumes as books of reference, but I note with eager approval the author's pledge that his work shall not close without a copious index (*einen ausführlichen Register*). This promise constitutes an additional reason to wish the distinguished author well and quickly through the remainder of his laborious task.

REGINALD W. MACAN.

BLAYDES' *ADVERSARIA*. PART II.

Adversaria in Comitorum Graecorum Fragmenta, scripsit et collegit F. H. M. BLAYDES, LL.D. Pars II. secundum ed. Kockianam. Halle, 1896. Pp. 360. M. 7.

DR. KOCK's edition of the *Fragments of Attic Comedy* has given the study of them a new stimulus; to be welcomed, not only because they are interesting in themselves, but because of their influence upon the later Greek literature—and, of course, upon the Roman. I am not thinking only of mere centos, Epistles of Alciphron and Aristænetus; but Lucian, for instance, the romance-writers, sophists, moralists, epigrammatists—Comedy was for these what Homer was for the tragedians. Comedy—especially the middle and the new—was the abundant spring that supplied them with themes and types and phrases.

This may be illustrated by a new example. In a tirade against women [Lucian] *Amor.* 42 ii. 443 (a sophistic σύγκρισις): τίς οὖν ὁ μετὰ τὴν τοσαύτην παρασκευὴν βίος; εἴθ' ἀπὸ τῆς οἰκίας ἔξοδοι, καὶ πᾶς θεὸς ἐπιτρέβων τοὺς γεγαμηκότας, ὧν ἐνίων οἱ κακοδαίμονες ἄνδρες οὐδὲ αὐτὰ ἴσασιν τὰ δνόματα—Κωλιάδας,¹ εἰ τύχοι, καὶ Γενετυλλίδας, ἣ τὴν Φρυγίαν² δαίμονα καὶ τὸν δυσέρωτα κομμὸν ἐπὶ τῷ ποιμένι. τέλειται δὲ ἀπόρρητοι καὶ χωρὶς ἀνδρῶν ὑποπτα μυστήρια καὶ—τί γὰρ δεῖ περὶ πλέκειν;—διαφθορὰ ψυχῆς. Sommerbrodt, the latest editor, places an asterisk against πᾶς θεὸς and remarks 'π. θεατρῆς ΩΓ Harl. Obscura haec neque ullo modo adhuc

illustrata. Hoc solum constat interiisse haud pauca ante ὧν ἐνίων.' There is no omission, nor should the meaning be in doubt. The complaint is of the luxury of women and their addiction to orgiastic forms of worship (Ar. *Lys.* 387—396); the γυνὴ φιλέσδοις makes every imaginable obscure divinity (Scholl. on *Lys.* 1 and 389) an excuse for going abroad. The phrase is from Menand. 601 (quoted by Strabo 297):

ἐπιτρέβουσιν ἡμᾶς οἱ θεοὶ
μάλιστα τοὺς γήμαντας· αἰεὶ γὰρ τινα
ἄγειν ἑορτὴν ἔστ' ἀνάγκη.

and (as I have indicated) more from the same source is probably embedded in the passage. The *shepherd* is Attys (Theocr. xx. 40) or Adonis (iii. 46, xx. 35), to whom the MS. κῶμον is inapplicable: I have therefore emended it. (Cf. Lucian i. 233, iii. 646 of Attys: of the *Adonia*, iii. 454, Ar. *Lys.* 396, Dioscor. *A.P.* v. 53, Plut. *Alcib.* 18, *Nic.* 13, Bion i. 81.)

English scholars, since the days of Porson and Elmsley and Dobree, seem to have done little in this region—Dr. Blaydes records conjectures by Prof. Ellis, Prof. Palmer, a few of my own—but every student of pure literature should be familiar with these remains and with what can be gathered from the Roman adaptations of Plautus and Terence.

Like all Dr. Blaydes' work, this volume might with advantage have been many times less in bulk, so full is it of repetition and unprofitable remarks. Readings, conjectures, comments, are needlessly tran-

¹ Ar. *Lys.* 2, Nub. 52 Blaydes.

² Pollux iii 11, Diog. Laert. vi. 1, 1.

scribed from Kock, often without any criticism. But from Dr. Blaydes we must take what we get; and if we do not now get much of real importance, it must be remembered that he had already had his say in a volume (published in 1890) of *Adversaria* on Meineke's edition. His long and devoted study of Aristophanes has given him familiarity with the diction of Comedy; shown here chiefly in collections of similar forms, as pp. 25, 51 on *περιόντας*, 52 *ἀγύναικος*, 67 *σιπύη*, 79 *γαστρίστερος*, 142 *ἐγγελεύδιον*. It has not, however, given him a sure hand: p. 351 in a fragment from Synes. p. 728... *πλείν ἢ παλαστή... σοφώτερος*. 'Qu. *πλείον παλαστής* B. Crates 15 *ἄλλ' ἀντίθεσ τοι* ἐγὼ γάρ... 'Qu. *ἄλλ' ἀντιθέρε* B. This is a dialectical formula: Plat. *Gorg.* 461 E *ἄλλ' ἀντίθεσ τοι* Eur. *Herac.* 153 *φέρ' ἀντίθεσ γάρ* and similarly *Or.* 554, Dem. 385, 13. Alexis iii. 7 *Φίλας Ἀφροδίτης*. 'Miseris genitivum sic formatum.' This *Φίλα* was a celebrated person: another, mentioned by Philetæus. 9, 5, was a famous hetaira; as was *Λύκα*, mentioned by Timocles 25, 2, Amphis 23, 4 *Λύκα*, where B. says 'Qu. *Λυκίδε*.' A long list of feminine name-forms in *ā* is given by Cobet *V.L.* 202. Alexis 270, 5 A. *Διὸς σωτήρος*; B. *οὐκ ἄλλον μὲν οὖν*. 'Qu. *θεοῦ*, aut *οὐκ ἄλλως λέγω*. Particulae μὲν οὖν (imo) correctioni inserviunt.' That is one effect, but it is only one of the general sense, an emphatic 'indeed': e.g. Plat. *Theæt.* 189 E Σ. *οὐκ ἀνάγκη...*; Θ. *ἀνάγκη μὲν οὖν*. It is constantly used in assent after πάνν, παντάπαστι, κομίδῃ. Another unfortunate lapse is on Heniochus 4, 3 (ii. 432) [*προσσελαληκνέαι*] *προσσελαληκνέαι* scribendum foret, si a *προσσελαλύν* deductum esset. Sed corrigendum proculdubio *προσσελαλκνέαι*. Dr. Blaydes holds very unsound views upon the use of the article: Crates 27, 2 *ἐπὶ κοχωνῶν τὰς τρίχας καθειμέναι*. 'Omitti nequit articulus ante *κοχωνῶν*.' Autocrates I, 4 *κἀνακροῦνσαι χερσίν*. 'Qu. καὶ κροτοῦσαι ταῖν χερσίν. Postulatur enim articulus.' Alexis 270, 3 *ἦν γὰρ... παλαιόν, ὅτα συντεθλασμένον*. 'ὅτα sine articulo posuisse poetam miratur Kock. Nempe eandem ob causam quod ὦ ante *Εὐριπίδῃ* et *Αἰσχύλῃ* omittēbant, durioris crasis evitandae causa.' That does not account for Plat. *Protag.* 342 C *ὅτα τε κατὰγονται*. The reason is, these are established combinations of words, grown almost into one: the compounds they represent exist, *ὠτοβλαδίας*, *ὠτοκάταξις*. The remark on Pherecr. 145, 6 *ἄνῃρ*. Mendosum, nam requiritur articulus' is an oversight, for it is plainly the predicate as in v. 17. He strangely mis-

apprehends, too, the use of *ὅδε* and *οὗτος* without the article: Telecleid. 35 *τίς ἦδε κραυγή*; 'Articulum desidero.' Diphil. 46, 3 *τόνδ' ἴδεν ἄσπὸν*. 'τονδὶ δὲ ναστὸν sagaciter Heringa. Sed articulum τὸν desidero. Leg. *τονδὶ τε ναστὸν*' (a characteristic inconsistency!). Now, the article is usually absent with *ὀδὶ*, *οὗτοσί*, because they are *deictic*; and when *ὅδε* and *οὗτος* are used in a *deictic* sense, the article is not required. Sometimes the absence of article indicates imitation of tragic *σεμνότης*: as Menand. 610 *νῦν δ' ἔρπ' ἀπ' οἴκων τῶνδε*: cf. Eur. *Hel.* 478. Antiphan. 176, 2 *δύναιτ' ἂν ἐξελεῖν ποτ' ἐκ τῆσδε στέγης* is Porson's correction of *ἐκ τῆς στέγης*: 'Sine articulo addito! Qu. *ἐκ τῆς οἰκίας*' is Dr. Blaydes' comment, though Kock notes 'tragicam gravitatem adfectari recte monuit Meineke.' In Ar. 268, therefore, *ἀνοίγέτω τις δώματ'*, I do not agree with B. in thinking Dobree's *ἀνοίγέ τις τὰ δώματ'* probable. Kock rightly remarks 'sine articulo *τραγικώτερον* sonat,' and that Tragedy is imitated the use of the word *δώματα* is enough to show. In Epicharmus 6 *ὄπωπα* is tragic: cf. Aesch. *Eum.* 56, *fr.* 155, Herodas i. 33. The cook, as usual, is using grandiloquent language. Comedy is full of such burlesque, to which critics are not always sufficiently alive. The flavour is lost unless we appreciate the heightening of diction. But there is no such burlesque in Aristophan 13, 9 (ii. 281) *φθείρας δὲ καὶ τρίβωνα τὴν τ' ἄλυσίαν οὐδεὶς ἂν ὑπομείνει* where B. proposes '*τρίβωνας* (vel *τριβώνι*) ἢ δ' *ἄλυσίαν*. Offendit enim singularis *τρίβωνα*'. On the other hand, in Anaxilas 18, 7 the forms indicate, as I had¹ pointed out, that 'Εφεσσία *γράμματα καλὰ* is a quotation. Kock (to whose illustrations add Diogenian. iv. 78, Schreiber *Atlas*, fig. xii. 2) suggests *καὶνὰ* on the ground that *καλὰ* is 'apud Atticos incredibile,' as though 'Εφεσσία were an Attic form, and Dr. Blaydes *κρυπτά* or *γραπτά* (p. 146), or *γραμμάτι' ἄττα* (p. 335).

It is plain we cannot take Dr. Blaydes for a safe guide; nor does he appear anxious that we should, with such temerity are his guesses made: e.g. *πλείον' ἀγαθὰ κτήσομαι* for *κτῆσομαι τοῖχον ἄρας* Philem. 116, 4, *τὸ φατίζόμενον* for *τὰ τῶν κακῶν* in Alexis 266, 1 *μὴ ὦρασι μετὰ τῶν κακῶν ἴκοιτο*, *παφλάζει* for *βαύζει* Cratin. 6, 1, *πενόμεθα* for *ἀλύομεν* Alexis 116, 3, *πέμπουσι* or *φέρουσι* for *κάνουσι τε τὰ πολυτελῆ ταῦτα δειπνα* in Lucian *Charon* 22 (*adesp.* 128). In these last three cases no alteration is required at all. But Dr. Blaydes is

¹ *Journal of Philology* 46, p. 280.

somewhat easily puzzled; as by Cratin. 274 the κύρβεις of Solon and Draco οἷσι νῦν φρύγουσιν ἤδη τὰς κάρυς, where he conjectures (p. 282) 'ἀλοῦσιν aut aliquid simile.' They are used for firewood: cf. the oracle in Hdt. viii. 96 Stein. 'Plat. 196 ἀνακογ-χυλιασθόν] lege ἀνακογχυλιάσασθ' aut ἀνακογ-χυλιασμόν (Anglice, a gargle). Vulgatum non intelligo.' φάρμακον is understood, as with the synonymous ἀναγαργάλικτον, ἀναγαργάριστον, and χριστόν, πιστόν, etc. Blomf. P.V. 488. The suggestions that commend themselves are of a soberer quality, as Ar. 135 ἐγὼ δ' ἀπολοπίζω γε for τε. Antiph. 47, 6 ἡδὺ τι <τὸ> κοινόν ἐστιν (where for οὐ χωρὶς read οὐκ ἔρεις?). Amphis 11 interrogation at end of v. 2. Amphis 28 εἰς [τὴν] ἐσπέραν. Philem. 246, 8 εἰ γὰρ <ὁ> δίκαιος κάσεβῆς ἔξουσ' ἴσον for κάσεβῆς ἔξουσιν ἐν.

Still, his remarks have often the value of calling attention to doubtful passages, a few of which I notice where I have something to contribute.

In Cratin. 364 should be read πισσοκωνίας ἀρήν (a form attested by Pollux vii. 184, Phryn. Bekk. An. 7. 12, Eust. 49. 28, 799. 32): cf. Hesych. Κωνῆσαι: ...πισσοκωνία γὰρ ἡ νῦν πίσσα ἢ χρίοισι τὰ παρίσθημα τῶν προβάτων.

Pherecr. 10, 4 ὥστε τὴν κόμην ὑπηχεῖν θιγανουσῶν τὰς μύλας: περιαιγουσῶν Kock, σὺτ' ἀλουσῶν ταῖς μύλαις B. Read θρυγα-νωσῶν τὰς μύλας 'scrapping': see Dr. Blaydes' collection on Ar. Eccl. 34, with which cf. Thesm. 481.

Pherecr. 70, 3 ὦ καταράτ<οτ> ἐνέχεας;

Eupolis 259 ἐγὼ δέ γε στίξω σε βελόναισιν τρισίν 'quid tamen tria illa stigmata sibi velint nescio.' K. 'Sc. tribus litteris ΔΙΠ' B. But this is not favoured by Plut. Artox. 14 προσέταξε διαπεῖραι τρισὶ βελόναῖς τὴν γλῶτταν (cf. Dion Cass. xlvii. 1 of Fulvia ταῖς βελόναῖς αἷς εἰς τὴν κεφαλὴν ἐχρήτο κατεκέντησε Cicero's tongue).

Ar. 350 κεραμενομέναι[s] κοτύλα[ι]ς με-γαλα[ι]ς B.; κεραννυμέναῖς or -αῖς seems more likely.

Ar. 596 (Ath. 444d) ἡδὺς γε πίνειν οἶνος 'Ἀφροδίτης γάλα, being always so printed, does not appear to me to have been understood. Kock says 'οἶνος ἡδὺς πιεῖν prae-dicatum est,' implying that 'A. γ. is the subject. I think there should be a comma at οἶνος (ῶνος? as Eur. Cycl. 555): 'Excellent wine! milk of Aphrodite!' an hyperbole (not like the metaphor ἀσπίς 'Ἄρεως φιάλη' quoted by Aristotle; which B. compares). Cf. Romans Grecs p. 36 Lambros εἶπες ἐκένην τὴν βαφὴν...τῆς Ἀφροδίτης αἷμα. Of the same class are Διὸς ἐγκέφαλος (Ephipp. 18, 7

Kock), the Pope's eye, Liebfraumlch, Lagrima Cristi.

Plat. 69, 5 τῇ παιδί τοὺς αὐλοὺς ἐχρῶν ἤδη προχειρίζαι με is nearer the MS. προ-χειροῦς εἶναι than Meineke's προχειρίζασθαι. B. approves Cobet's τὴν παιδα τοὺς αὐλοὺς ἐχρῶν ἔχειν πάλαι προχειροῦς.

Plat. 169

καὶ τοσοῦτον εὐερίας ἀπολέλανχ' Ὑπέρβολος ὥστ' ἀθλιωτάτος ἐστι (ἀθλιωτάτῃ Suid.).

B. finds nothing to substitute; ἀλεώτατος seems likely.

Callias 1 (i. 693) κέρδος αἰσχύνῃς ἄμεινον ἔλκε μοιχὸν ἐς μυχόν is rightly explained by Leutsch (quoted by K.), and may be illustrated by Dem. 1367. 3-13. Cobet's ἐκ μυχοῦ is mistaken.

Antiph. 277

ἂν μὲν περιάμενος ἄρα πέπερί τις φέρῃ.

Eubul. 82, 7

ἄμα δὲ λαβοῦσ' ἡφάνικε πηλίκον τινα οἴσθε μέγεθος ἄρεσιαν; μέγαν πάνν' καὶ ξηρὸν ἐποίησ' εὐθέως τὸν κύνθαρων.

May the word be ἀρυσίαν? Cf. Hesych. <Ἀρύσεις καὶ> Ἀρύστεῖς: τὰς ἀπνευστὶ πόσεις. τὰ δὲ αὐτὰ καὶ Ἀρυστήρας καὶ Ἀρυστήχους ἐκάλουν. Soph. fr. 697.

In Alexis 172 the typical bombastic cook is boring his hearer with details of the art (a scene like that of ¹Sosipater iii. 314, Nicomachus iii. 386). 'We shall have,' he explains, v. 13

ἐν ποτηρίῳ
γλυκύν—τὸ τοιοῦτον γὰρ αἰεὶ πως μέρος
ἐπιπαίζεται—κεφαλῇ δὲ δειπνον γίνεται

whereupon the impatient listener interrupts, ἀνθρώπ', ἐπιπαίξε' μόνον ἀπαλλάγηθί μου. 'Leg. ἄνθρωπε, παίξε' says B. No: it is a contemptuous quotation of the word ἐπιπαί-ζεται, just as in v. 7 on the remark ἔρια μὲν ποιῶμεν—the hearer exclaims ἄνθρωπε, ποιεῖ λευκὰ καὶ βλέπ' εἰς <δδόν>. It is exactly like Aesch. Theb. 1035 XO. τραχὺς γε μέντοι δῆμος ἐκφυγὼν κακὰ. AN. τράχυνε'...Ar. Eq. 469 A...χαλκεύεται. XO. εὐ γ' εὐ γε, χάλκευ' ἀντὶ τῶν κολλωμένων.

Diphil. 32, 6 εἰὼν ἀπολαίνει τοῦτον ἤδη τὸν βίον, where B. accepts Kock's ἀποβάλλειν, I do not doubt that τοῦ βίου should be read.

Menand. 173, 4 ἱκανόν ἐστι τῷ βίῳ? for κοινόν, which B. is right, I think, in calling 'vitiosum.'

¹ Journal of Philology 46, p. 284.]

Menand. 304 εἶτα τὴν ποιηρίαν | ἀτυφίαν νομίσαντες ἔξουσιν ποτὲ | πέρας; (or ποτε after ποιηρίαν).

I withdraw my¹ suggestion (which B. quotes) on Menand. 310 αἰὲ νομίζονθ' [v. l. νομίζεθ'] οἱ πένητες τῶν θεῶν, interpreting it now by Hom. ζ 207 = ξ 57 πρὸς γὰρ Διὸς εἰσιν ἅπαντες ξείνοί τε πτωχοί τε. Cf. ε 448. Apoll. Rhod. ii. 1132 Διὸς δ' ἄμφω ἱκέται τε καὶ ξείνοι.

Menand. 402, 1 εἰς ἀμφοτέρα νῦν (which is, of course, right) is read in the *Theaur. s.v.* ἀμφοτέραις.

Menand. 472, 7 τρόπος τὸ πείθον.

Menand. 607 I assign to the Μεσορηγία, because the practices described here by Plutarch are exactly those attributed to Ψάλλος by Marc. Arg. *A. P.* vii. 403, and Ψάλλος is recorded by Suid. and Phot. as a proper name in the Μεσορηγία.

Menand. 687 (= Trag. adesp. 507 Nauck) read ὦ δέσποτ', ἀλλ' ἔξεστι...for ἀναξ ἔστι ('Qu. εὐ ἴσθ' B.). Plat. *Euthyphr.* 3 C ὦ φίλε Εὐθύφρων, ἀλλὰ . . . Pind. *O.* vi. 22.

In Menand. 711 μηδέποτε πειρῶ στρεβλὸν ὀρθῶσαι κλάδον, | οὐκ ἦν ἐνεγκεῖν ὅπου φύσις βιάζεται Jacobi conjectured φύσιν δ' ἐνεγκεῖν οὐ φ. β. B. suggests οὐκ ἔστι κάμπτειν οὐ or κάμψ' ὅπου. Perhaps σ ν κ ἦν δ' ἐνεγκεῖν ἦ φ. β. The similar fragment, adesp. 182 οὔτε στρεβλὸν ὀρθοῦται ξύλον οὔτε γεράνδρον μετατεθὲν μοσχεύεται may be simply altered to γεράνδρον τ' οὐ (one of Dr. Blaydes' suggestions), since οὔτε...τ' οὐ is a correct consecution.

Macho 2, 9 a cook says, speaking of the *plat* in metaphors from music,

ὥσπερ λύραν ἐπίτευ' ἔως ἂν ἄρμόσῃ
εἴθ', ὅπότεν ἦδη πάντα συμφωνεῖν δοκῇ[s],
εἴσαγε διὰ πασῶν Νικολάδας Μυκόνιος.

Dr. Blaydes is, I think, upon the right track with ψάλλε or κροῖε. Perhaps κραγέ or παῖε. The last two words may mean 'like N.'; or, as I suspect, it is the name of the triumphant song he is to strike up. Dr. Blaydes' suggestions are καὶ κάλει τοὺς Μυκόνιους or ἵνα πάρωσ' οἱ Μυκόνιοι.

¹ *Journal of Philology* 46, p. 274.

Strato 1, 4 read πεπορισμένος γὰρ ἔστι for πάρεστι.

In a papyrus fragment, adesp. 104, the speaker is testifying how he has received light and salvation from a philosopher. 'Before,' he says,

5 ΠΑΓΓΗΚΤΟ τὸ καλὸν, τὰγαθόν, τὸ σεμνὸν
<ἦν>,
τὸ κακὸν τοιοῦτον ἦν τι μου πάλαι σκότος
περὶ τὴν διάνοιαν...

'but now,' he goes on,

11 ἀναβεβίωκα' περιπατῶ, λαλῶ, φρονῶ,
ΤΗΝ (ζῶ' ?) τηλακοῦτον καὶ τοιοῦτον ἥλιον
νῦν τοῦτον εὖρον...

(so I conjecture) 'such a sun of illumination have I found in him.' In v. 6 (cf. Philem. 71, Apollod. Caryst. 5, 5, Amphis 6) B. suggests ἀγνοῖα. Since the letters ΓΗΚ are said to be doubtful, the truth may be ΠΑΝΤΑΥΤΟ; πᾶν ταῦτ'...ἦν 'they were all one.'

Clem. Alex. p. 842 quotes adesp. 341 ἂν μὴς διορύξῃ βωμόν ὄντα πήλινον | κᾶν, μηδὲν ἄλλ' ἔχων, διατράγῃ θύλακον, you take it for an omen.' B. remarks 'τοῖχον recte Naber. Mures enim toichorýchous perfordere parietes, aras autem non ex luto aut argilla facere morem fuisse, neque, si ita mos fuisset, causam fuisse cur eas arrodere.' There is no reason to presume that an altar was never made of clay. One of clay is supposed here, because they could hardly tackle one of stone. It is several times recorded as an actual portent that mice had gnawed gold in temples: Liv. xxvii. 23. Plut. *Marcell.* 28, *Syll.* 7; a gold crown Liv. xxx. 2. Cf. *A. P.* ix. 310.

In Liban. iv. 836 (adesp. 1549) read ἐξ ὄτουπερ ἐγένον for ἐξ ὄτου παρεγένον, and make the same correction in Liban. *Epist.* 762 for ἐξ ὅσον περ.

Among the various fragments of verse which he adds at the end of his book, Dr. Blaydes does not claim that much is new, and most of them I have seen before; but Dr. Kaibel may find them of service for his promised edition.

WALTER HEADLAM.

MUELLER'S DE RE METRICA.

Luciani Muellieri *De re metrica libri septem.*
Editio altera. Petropoli et Lipsiae. 1894.
M. 14.

PROFESSOR LUCIAN MUELLER'S new edition of the *De re metrica* is in every way worthy of

his reputation. The first edition of this valuable work was rather inadequately equipped with indices, a deficiency which has now been supplied. The old edition, besides a table of contents, contained two

indices: I. a list of authors quoted, with the editions to which the references were made; II. a miscellaneous index of words and authors emended or illustrated. The new edition has three indices: I. a full summary of the contents of the book with running references to the pages; II. an enlarged general index; III. a list of authors quoted with the editions used. At the end is a table of contents. A further improvement has been made in the body of the work by the omission of a large number of the writer's own conjectural emendations, which are now to be found in his published editions, notably of Ennius, Lucilius, Phaedrus, and Nonius. The whole text has been much altered and rewritten; the type is finer and bolder than that of the first edition; statements of a general nature are now printed in spaced type.

A peculiar interest is lent to the book by the preface in which the veteran Latinist introduces his revised labours to the world. There is charm in the pathetic pleasure with which he who has done so much for the Roman poets contemplates the accomplishment of his task. "*Qui cum totus subiaceret oculis nostris nitidissime typis expressus et emendatissime, sicut ducem quendam ueterem ferunt post captam hostium urbem, non potui temperare a lacrimis, partim gaudio rei perpetratae, partim recordatione malorum, quae per hos triginta annos, grande aetatis humanae spatium, acciderunt uel antiquitatis studiis uel nobis, qui eorum, si non magna, certe aliqua pars fuimus.*"

On the continent generally, as in England, the utilitarian requirements of the age threaten the supremacy of classical studies. This fact Professor Müller eloquently deplores. The illiterate masses, he says, are everywhere straining after more political power; and the growth of an unreasoning democracy means the downfall of classical education, and with it of art and culture, of elegance and grace. Again the ever-growing poverty of students forces them to turn their brains at once into money; thus knowledge ceases to be pursued for its own sake, and the classics are displaced by physical science and modern languages, which attract by the immediate bribes they have to offer. Further, in Germany the increasing study of mediaeval writers, the outcome of Teutonic patriotism, diverts attention from the classics. Against this condition of education Professor Müller protests, as his manner is, with no uncertain note. He argues that now, if ever, the severity of a classical training is needed to refine and purify the

degraded public taste. It is only the absence of classical feeling which renders possible the existence of a realistic school of writers of the Zola type. Homer, Sophocles, Cicero, and Horace are the best antidotes to their tawdry blandishments. '*An, si rectiore staretur iudicio, Zola et Sudermannus plurimique, qui secuntur eos, tantum potuere assequi famae ac laudis? quid? theatra, quae olim plurimum contulerunt ad excolenda ingenia et exornanda, quibus iam solent perstrepere fabulis?*' (p. vi.). Again, he pleads for the incomparable superiority of the classics over the moderns as a curriculum; and emphasises the inferiority of mediaeval writers to those same classics. Who, he asks, could seriously set the *Nibelungenlied* or *Tale of Gudrun* against the *Iliad* and *Odyssey*, or *Parsifal* against the *Aeneid*? English lovers of antiquity will read this preface with sympathetic delight.

The classical training being the necessary basis of a liberal education, Professor Müller goes on to show that for the proper appreciation of the undying poets of paganism a thorough mastery of their metre is necessary; for form is to the poet as important as matter, and form is the great fosterer of clear thought and appropriate language. This constitutes the justification of his elaborate treatise.

I cannot leave the preface without protesting against its acerbity of tone. Professor Müller is an avowed enemy of the followers of Lachmann and Ritschl, but he need not have paraded his hostility afresh, especially as in the body of the work he has omitted much of the vituperation which appeared in the first edition; for example, the attack on Vahlen and Ribbeck, pp. 80-81 of the first edition. It is pleasing to turn from his acrimonious language to the feeling tribute which he pays to Count Tolstoi's services to the cause of education in Russia (pp. viii-ix).

The scope of the book remains essentially unchanged. The metres of Plautus and Terence are not treated, partly because many questions with regard to them are of so controversial and obscure a character as to defy satisfactory settlement, and partly because their metres are of a different type from those of the followers of Ennius, who reproduced the Greek prosody. The poets treated are of two classes, the classical and the Christian; they are enumerated in detail, Terentianus and Boetius being regarded as standing midway between the two. The book opens with a survey of the systematic

study of metres, which began with the sophists, who, after the decline of Greece, taught the various mixed races, who though speaking Greek required instruction in metres which they no longer understood instinctively. The quantities of syllables, it is shown, were regularly taught in Roman schools from the fourth century B.C. onwards. An interesting passage of new matter, pp. 8-10, emphasises the influence of the *collegia poetarum* and of public recitations on the study of metre. Müller now abandons (ed. 2 p. 12 = ed. 1 p. 14) his former contention that there were two classes of Roman metrists, the better, whose works have perished except a few fragments, and the worse, of late date, who though lacking in merit, have survived on account of their popular character. He now considers that all the ancient metrists worked on the same lines; that they all originated when Greek and Roman literature were still flourishing; and that all their work was trivial and uncritical, containing more of falsehood than of truth.

The work consists of seven books. Book I. *De studiis poetarum Latinorum metricis*, reviews the Roman poets in metrical relation to their Greek originals. Book II. *De pedum observantia*, discusses the different feet employed, and closes with a series of emendations of Seneca's tragedies and Silius Italicus. Book III. *De caesura*, treats of caesura and accent. Book IV. *De vocalibus inter se concurrentibus*, discusses hiatus and elision. Book V. *De vi consonarum coeuntium et de productis uel correptis finalibus*, contains the laws of quantity. This book has been largely rewritten, and here the author's studies of Ennius and Nonius have given him a wider grasp. Thus on p. 401 = 327 ed. 1, after quotations from Ennius of lines where final syllables in *ar*, *or*, *us* are lengthened, occurs the following addition: 'eximendum putavi illud quod legitur apud Nonium pg. 120 s.l. Hora:

Quirine pater, ueneror Horamque Quirini.
nam ibi cum non Iuventas dea significetur,
ut uult Nonius, sed coniux Romuli inter
deas recepta, cuius nomen corripitur ab
Ouidio Metam. xiv. 851, qui haud dubie
Ennii secutus est exemplum:

mutat Horamque uocat, quae nunc dea
iuncta Quirinost,
scribendum potius:

teque, Quirine pater, ueneror bene Horam-
que Quirini

uel

teque, Quirine pater, bene Horamque
Quirini.'

NO. XCII. VOL. X.

This passage is a fair specimen of Müller's method, which ignores too much the views of others. Baehrens' reading of the line (*Fragmenta Poetarum Latinorum*, p. 70).

teque, Quirine pater, ueneror, Hora, teque,
Quirini,

is at least as ingenious as either of Müller's proposals; but Baehrens is not a favourite with the St. Petersburg professor. Book VI. *De mutatis aliqui quantitibus syllabarum et de uerborum tmesi et enclisi*, treats of changes of quantity, tmesis enclisis and proclisis. Book VII. *Observationes Grammaticae*, deals with poetic grammatical peculiarities of form. The new edition closes, like the former, with four special treatises, of which the three last are materially the same, while the fourth has been entirely remodelled, and is entitled now no longer *De Lucilii Varronisque et Phaedri iambis ac trochaeis Italicis*, but *De uersibus dactylicorum Italicis*.

In conclusion, the work has been greatly improved and augmented; but its usefulness is somewhat interfered with by a fault to which I have already referred; the author's egotism leads him to neglect opinions differing from his own, and very little reference is made to other writers. Thus in bibliography the treatise remains singularly weak, a rare thing with German writers. I give one instance: the account of the galliambic metre (pp. 174-176) is more satisfactory than that in the first edition (pp. 159-160); but the dogmatic assertion 'Catullus numquam admisit ionicum a minori' ought not to have been made without some reservation, as many competent judges think otherwise. I agree with Lachmann in holding it certain that Catullus did admit the ionic a minori foot in lines 54 and 75 of the *Attis*; and it is quite possible that he did so in line 18. The note in my edition might have abated Müller's confidence, but perhaps he has not seen it, as he shows hardly any acquaintance with English scholarship. The work of Munro on Lucretius, Catullus, and the Aetna, of Ellis on Catullus, Manilius and Avianus, of Postgate on Propertius, and of myself on Ovid meet with no recognition from him. The only English book he seems to know is Ellis's *Orientius*, which was published in Austria. But in spite of its shortcomings the book is a remarkable contribution to Latin scholarship; the author's industry, learning and lucidity deserve the highest praise.

S. G. OWEN.

¹ There is a mistake in the table of contents, p. 650, 'Liber septimus' has been omitted. Also on p. 181 *Persas* is a misprint for *Parthos*.

DITTRICH'S *AETIA* OF CALLIMACHUS.

Callimachi Aetiorum Librum I., prolegomenis, testimonis, adnotatione critica, auctoribus imitatoribus instruxit EUGENIUS DITTRICH.
Leipzig: Teubner, 1896. 2 Mk.

THIS dissertation on the first book of the *Aitia* of the poet Callimachus forms part of the twenty-third supplemental volume to Fleck-eisen's *Jahrbücher*, and extends to fifty-two pages. It includes a copious index nominum, which greatly adds to its value.

Dr. Dittrich has spared no trouble to obtain the *sources* of the fragments, out of which he reconstructs the first book of the *Aitia*, in the most correct form. I mean that he has, wherever possible, procured new collations of the best MSS. of the various authors, Ammonius, Choeroboscus, the *Etymologicum Magnum* and four other lexica, Galen, Stephanus Byzantius, &c., in which the *Aitia* are cited. See the list on pp. 204-5.

The plan of the treatise is as follows. First, the main discussion, in which the fragments expressly assigned to Bk. I. are arranged in something like probable order, and other fragments, quoted as by Callimachus, but not attributed to the *Aitia*, are added as finding a place naturally in the series. In this section Schneider's *Callimachea* is naturally the ground-work; as naturally, the conclusions of Schneider are accepted with many reservations. Every one who has followed the literature of Callimachus knows how far below the level of Schneider's *Nicandra* is this his latest work, indispensable as it notwithstanding is, partly from the vast grammatical erudition which distinguished Schneider among contemporary philologists, partly from the diligence with which he has recorded the opinions of other scholars, not only great names like Bentley, Blomfield, Näke, Gaisford, Meineke, Bergk, but men who like Hecker, Bachmann, Dilthey, &c., have made a special study of Alexandrian literature. Next to Schneider, Dr. Dittrich gives much weight to the opinions of Hecker, whose masterly work on

the Greek Anthology is not so well known in England as it deserves to be.

The Dissertation (p. 167-200) is followed by an *Argumentum Lib. I. Aetiorum* in which Dittrich draws out in sequence what he imagines to have been the plan of the poem. He considers it to have contained nine Elegies, the connecting thread in all of which was the story of Io's wanderings. Interwoven with this were other favourite stories of Greek mythology, e.g. Coroebus, Linus, the death of Ajax son of Oileus, the Oenotzopae, the legends connected with the building of Troy (pp. 201-204). Then the chief MSS. containing the fragments, and an epigram, first printed by Hagen, which gives a catalogue of the works of Callimachus, where I find a verse very interesting to students of Ovid's *Ibis*—

σκόπτω δ' ἐπ' ἀραίς Ἴβιν Ἀπολλώνιον,

on which epigram Reitzenstein has written in *Hermes* xxvi. p. 308 sqq.

The actual fragments of *Aitia* I, with the authors who cite them, the readings of the best MSS., and the conjectures of scholars, are contained on pp. 206-214.

I have found this work interesting all through, and though often disinclined to argue from particular fragments to conclusions as bold and decided as Dr. Dittrich's (some are so short that no argument can be drawn from them), am very grateful for this new excursion into a somewhat neglected field. In the treatment of his subject our author has worked in not a little Latin poetry, especially Ovid. I could wish that the *Ibis* scholia were better than they are; but even they have found a recognition, though a somewhat dubious one, in this treatise; and it is probable that if Egyptian researches recover any portion of the *Aitia*, we shall come across many old friends, familiar to us from the *Ibis* and the *Metamorphoses*.

ROBINSON ELLIS.

WACKERNAGEL'S *ALTINDISCHE GRAMMATIK*.

J. WACKERNAGEL, *Altindische Grammatik*.
I. Lautlehre, Pp. lxxix., 343. Vandenhoeck und Ruprecht. Göttingen, 1896.
8 Mk. 60.

SANSKRIT, which was for a time the spoiled child of the Indo-Germanic family, has of recent years for a variety of reasons fallen somewhat into neglect, and, while the majority of the sister languages have met with full treatment from the comparative standpoint, in the case of Sanskrit either the interest or the courage has been lacking for such an undertaking. This gap is now in the course of being most admirably filled by Professor Wackernagel's *Altindische Grammatik*, the first volume of which has now been issued, and which promises to be one of the most important works in the field of Indo-Germanic philology that has appeared for a long time. In fact the width of knowledge, sobriety of judgment and clearness of exposition displayed in it make it a model of its kind.

The introduction furnishes an excellent sketch of the history of the language, in which are pointed out the various influences that have helped to mould the literary speech. The possibility of the influence of the popular dialects is always kept in view, and is applied, for instance, most ingeniously to the explanation of *kuru* by the earlier *kṛnu* (p. xviii). The main part of the present volume deals with the development of the Indian sounds from the Indo-Germanic. The fulness of the references here makes this a veritable treasure-house of information on various points of Indo-Germanic phonology. If we were disposed to quarrel with Professor Wackernagel, it would be rather for giving too much than too little. Surely it would have been kinder to the memory of the scholars of the past to have allowed many of their suggestions to rest quietly in their graves.

It is impossible here to deal at length with the many problems suggested by the book. It may be of interest to indicate Professor Wackernagel's attitude to some of the questions of the day. With Brugmann and others he holds that in certain cases Skr. *ā* corresponds to Idg. *o*; an interesting example is *tvāt-pitāras*: *ā-pāropes* (p. 15). For the reduced form of the long vowels *ā*, *ē*, *ō*, he suggests (p. 18) as Idg. the Greek vocalism, *a*, and a second *e*, *o*. The long sonant liquids and nasals, against which Schmidt recently delivered so effective a polemic, still appear, but they are strictly confined to the so called *udatta* roots, where, in the absence of any certain knowledge of the Idg. sounds which became in Skr. *īr* &c., they may perhaps have a certain value as algebraical symbols; they are not used as a sort of *deus ex machina* to explain any awkward case of vocalism. The changes *gh* > *gdh* etc. are given as Indo-Germanic (p. 131). Here some mention ought surely to have been made of the other view, for that the change was Idg. is at least far from certain, see now Kz. xxxiv. pp. 461 sq. Streitberg's explanation of the 'dehnstufe' is rejected (p. 68).

One or two small points may be noticed. In the explanation of *āyanna* there seems to be a contradiction between § 8 a and § 175 b. In § 127 b a of the I sg. perf. act. is derived from *a* or *m*. Unless the Irish form is to be separated from the Aryan and the Greek, the latter alternative is impossible. p. 129 does not *ka* in *āpāka*, *abhika*, *paçcā* etc. come from the stem *ōg*, cf. Schmidt, Plur. pp. 388 sq.?

We trust that Professor Wackernagel may soon give us the rest of his Grammar. The volume on morphology should be very interesting, for there is evidence in the present volume that the writer by no means agrees with some of the theories now in vogue.

J. STRACHAN.

ARCHAEOLOGY.

FURTWÄNGLER'S *INTERMEZZI*
AND *STATUENKOPIEN*.

Intermezzi: Kunstgeschichtliche Studien von A. FURTWÄNGLER, mit 4 Tafeln und 25 Abbildungen im Texte. (Leipzig and Berlin: Giesecke & Devrient. 1896. Price 12 Mkrs.)

Ueber Statuenkopien im Alterthum von

ADOLF FURTWÄNGLER, Erster Theil; mit 12 Tafeln und mehreren Textbildern. (Aus den Abhandlungen der K. bayer. Akademie der Wissenschaft. Bd. xx. Munich. 1896. Price 8 Mkrs.)

So full of new material—and, needless to say, of new theory—are the five essays recently published by Professor Furtwängler

under the somewhat fanciful title of *Intermezzi* that they seem to call for a summary or analysis rather than a critical review. The book opens with the publication of the superb bronze head of Apollo belonging to the Duke of Devonshire's collection at Chatsworth. It is pleasant to note in this case, that if the honour of publication falls to a foreigner, the greater honour of discovery belongs to an Englishman. Michaelis had apparently not seen the bronze when preparing his *Ancient Marbles in Great Britain* and is content to mention it on the authority of Mr. Sidney Colvin as of 'late, somewhat heavy workmanship' (*op. cit.* p. 277). It was thus reserved for Professor Strong, the present librarian at Chatsworth, to divine in this head the creation of a Greek sculptor of the early years of the fifth century B.C. He was confirmed in his belief by Professor Furtwängler, to whom he courteously entrusted the publication of the bronze, thus paying a graceful tribute to the great scholar who has so assiduously called attention to the treasures contained in the private collections of England.

Furtwängler recognizes in the Chatsworth head an entirely new type—or more accurately, new characterization, of Apollo—less dreamy and melancholy than the Pheidian, less loftily conceived than the Myronian, but far surpassing in freshness and spiritual distinction the rustic heaviness of the Apollo of the western pediment at Olympia. So far critics will be unanimous. Less satisfactory, however, is Furtwängler's attempted attribution of the head to Pythagoras of Rhegion, for if we turn to our author's own earlier surmises with regard to that artist (*Meisterwerke der Griechischen Plastik*, p. 347 = Engl. ed. p. 171) we find that the athletic types which he grouped together with 'Pythagoras' as provisional label, are characterized by features directly opposed to those of the Chatsworth head. In discussing, for instance, the Perinthos head, which, together with the head of a boxer in the Louvre, he now especially selects for comparison with the head at Chatsworth, Furtwängler well defined 'the lifeless, perfectly horizontal line' of the mouth, the 'angular lids which produce a wholly unnatural effect, as though they possessed no power of movement.' Now the most salient feature about the Chatsworth head is the full, curving mouth, which the compression of the lips alone redeems from the reproach of sensuality; furthermore, the lids, though archaic in

treatment, betray the artist's search for life-likeness and correct articulation (this is especially clear from the profile view on Pl. II.). In the face of such positive and far-reaching divergence the stylistic affinities detected by Furtwängler in the shape of the crown, or the modelling of the brows, seem fanciful and even arbitrary. It would of course be absurd to limit a great artist to one type of head or to suppose that he would conceive a god on the same lines as an athlete, but when the attribution of the one type is itself only a hypothesis, we may hesitate before allowing it to draw in to the same artist on the ground of small superficial resemblances, a second radically different type.

It is certainly true that 'a significant artistic personality is concealed behind our Apollo.' A like vigour and terseness of execution can be found within the same period only among the figures of the Eastern pediment of the temple at Aegina. The Apollo with his hard-shut yet vibrant lips strikingly recalls the 'Herakles' of the pediment. One might almost fancy Onatas—if indeed Onatas be the master of the Aeginetan pediments—creating in later years for his celebrated Apollo at Pergamon (Paus. viii. 42, 7) a type like that of the Chatsworth head, in looking at which Furtwängler himself was reminded of the epithet *βοῦπαις* applied by a poet of the Anthology (ix., 238) to the statue of Onatas. But in the dearth of evidence, such speculations must for the present remain entirely idle. We may feel confident that 'the Master of the Chatsworth Apollo' strongly impressed his, time, was imitated and copied; his true personality will reveal itself all the quicker if we do not prematurely try to make him fill a special gap in the history of the Greek sculptors.

The second essay reopens the time-honoured question of the central group of the eastern pediment of the Parthenon. Furtwängler has gradually come to believe that the centre of the pediment was held by the dominating figure of the goddess, and he finds abundant reason for supposing that the torso Medici at Paris may actually be this figure. If he himself could once have looked upon this torso as only a marble copy from a bronze original of the Pheidian period this was owing to the stupid height at which the torso was then exhibited. From the days of Ingres, who caused it to be brought from the Villa Medici to the Beaux-Arts, up to our own, the torso has been almost unanimously connected with

the name of Pheidias. Now that it is accessible to close inspection it turns out to be nothing less than an original, closely related to the Parthenon marbles in conception, technique, and treatment of drapery. The torso Medici was certainly originally made for Athens, the marble—like that of the Parthenon—being Pentelic, and the figure having been copied on more than one Athenian votive-relief. Further, from the movement of shoulders, neck, and arms it was evidently a pedimental figure, and if head and helmet be restored in proportion to the torso a height is obtained precisely fitting the centre of the eastern pediment of the Parthenon. Certainly these would be strong reasons for attributing the Athena Medici to the pediment did not Bruno Sauer's drawings of the floor of the pediment (*Ath. Mittheil.* xvi. 1891, p. 59 ff; *Antike Denkmäler* i. pl. 58) seem at first entirely to preclude the notion. It will be remembered that owing to the presence of a long elevation or ridge ('*Randbank*') in the centre of the pediment Dr. Sauer had decided against a single central figure and reverted to the hypothesis of R. von Schneider, according to which two figures of equal importance, Zeus and Athena, disposed much as on the Madrid puteal, occupied the middle of the pediment. The central ridge was then explained by Sauer as marking the line of the footstool of Zeus, while of the two converging broad iron bars, whose clear traces may be seen on the central block (13) of the pediment, the northernmost was considered to have supported the heavy figure of Athena, the southernmost, together with the bar immediately behind it, the still heavier Zeus. These results were for a time accepted without reserve by Professor Furtwängler himself (*Meisterwerke* p. 243 = Engl. ed. p. 463). In face, however, of his growing conviction that the centre can only be satisfactorily filled by the figure of Athena, he now proposes to solve the technical question otherwise than Dr. Sauer: the central ridge or *randbank* by no means necessarily precludes a central figure; its object was rather, he thinks, to equilibrate a heavy, massive figure supported on both the broad iron converging bars. The necessity for the *randbank* is explained on the supposition that the bars though sunk into the floor of the pediment, yet rose somewhat above it. Thus the presence of a central figure can be thoroughly reconciled with the traces on the floor of the pediment. There is, however, one grave objection—brought forward by our author with his wonted

candour—to identifying this figure as the Athena Medici. The plinth of the torso shows distinct traces of having been fastened by means of dowels. But the drawings of Sauer reveal no corresponding holes in the floor of the east pediment. This difficulty Furtwängler attempts to surmount by the suggestion—thrown out for the rest with exceeding reserve—that some Roman despoiler had torn the figure from the Parthenon and borne it off to Rome to decorate some temple pediment, when the dowelling was first found necessary. The dowelling marks have at any rate the advantage of proving that the figure belonged to a pediment, while the theory of Roman spoliation would explain the presence of the torso in Rome; it really be from the Parthenon it would be difficult to understand how it got to Rome in more recent times. A fine drawing illustrates Furtwängler's present notion of the general effect of the pediment; by filling the centre with the figure of the goddess, he has assuredly imparted to the whole a unity and strength lacking in all previous restorations. The whole theory, however, is only put forward tentatively—it will be interesting to watch what alternative suggestions are offered as to the original purport of a pedimental figure made like the Medici torso of Athenian marble, copied on Athenian votive reliefs, and closely agreeing in style and proportion with the figures of the Parthenon.

A curious discovery has enabled Furtwängler to solve definitely the date and purport of the well-known frieze in Munich representing the 'Marriage of Poseidon and Amphitrite.' During a recent visit to the Louvre he found a further portion of this frieze—a relief of similar height, material, and dimensions, which like its companion at Munich was once in the Palazzo Santa Croce. The Paris relief shows a Roman general performing, amid his retinue and with the assistance of a priest, the solemn sacrifice of the *suovetaurilia* (the animals are quaintly represented in the inverse of the order suggested by the word and common on other monuments). The name of the general, who was also presumably the donor of the whole monument decorated by the frieze, is not far to seek. Long ago Ulrichs had shown that the Munich frieze must have belonged to the temple of Neptune in *Circo Flaminio* whose site was close to that occupied in modern times by the Palazzo Santa Croce. It is evident, therefore, that the sacrificing general can be

none other than Cn. Domitius Ahenobarbus who, in the years 35-32 B.C. on the expiration of his governorship of Bithynia built—or perhaps only restored,¹ the temple of Neptune, the god who is celebrated on the Munich portion of the frieze. Thus the newly recovered fragment fixes the date of the whole, and it becomes possible to discard the once popular view that the Munich frieze was to the art of Skopas what the frieze of the Parthenon was to Pheidias art. The frieze which he has thus completed Furtwängler attributes to an altar in front of the temple: this hypothesis, moreover, can alone account for the little pilasters which bound the sacrificial scene, and reappear in identical form on the Munich frieze, where they mark off the central scene in a length precisely equal to the scene of sacrifice. If we follow Furtwängler in assigning the Paris relief and the longer Munich scene respectively to the back and front of an altar, while the two shorter Munich slabs each adorned one of the sides, we not only understand the pilasters which are so worked as to display a return face, but obtain an altar according admirably with the extant ruins of the temple.

In the last essay Furtwängler tries to discover what event the Roman *tropaeum* at Adam-Klissi in the Dobrudscha was intended to commemorate. He refuses to follow Benndorf² in referring the scenes sculptured along the metopes and battlements to some exploit in Trajan's second Dacian campaign: the large inscription *Marti Ultori . . Traianus* etc., though found among the ruins, has nothing whatsoever to do with the *tropaeum*; Benndorf's theory necessitates a perverse and untenable explanation of those reliefs on the Trajan column celebrating the Emperor's journey to Dacia and his arrival³; most important of all, the barbarians represented at Adam-Klissi are of a type totally different from the Dacian; they wear narrow trousers, are generally naked from the waist up, their beards are long and their hair is combed into a knot at the side after the fashion recorded by Tacitus (*Germ.* 38) as characteristic of the German tribes. The clue to the real purport of the monument is its geographical position: as the *tropaeum Augusti* (La Turbie) on the spurs of the

Maritime Alps above Monaco, marked the conquest of the Alpine region by Augustus, as the *Tropaeum in Pyrenaeo* erected by Pompey after the Sertorian wars marked the boundary of the newly-conquered region, as Drusus and Germanicus marked the limits of their conquests by *tropaea* upon the Elbe and the Weser, so the *tropaeum* at Adam-Klissi must have been erected at a time when the Danube became the new frontier of the Empire, i.e. in the Augustan period. The lands on the right shore of the Danube were conquered by Marcus Licinius Crassus in 29-28 B.C., when the wild German tribe of the Bastarnae and the hostile peoples of Northern Thraciae were once and for all expelled from the region. Furtwängler believes, accordingly, that the *tropaeum* of Adam-Klissi commemorates this campaign, and that the sculptures of the metopes represent for the greater part scenes from the deadly battle in the forest when the troops of Crassus fell upon the unsuspecting Bastarnae and annihilated them, Crassus slaying with his own hand their King Deldon (Dio Cassius xxxviii. 10). Thus the Germanic type of the barbarians of Adam-Klissi receives a satisfactory explanation, while history gains for the German wars of Rome as impressive a monumental witness as the Trajan column is to the Dacian wars or the column of Marcus Aurelius to the Marcomannic campaigns.

The book closes with an excursus upon the too notorious 'Tiara of Saïtaphernes.' At greater length than was possible in the article published in *Cosmopolis*,⁴ the author shows whence the forger borrowed the motives of the tiara, without discrimination of style or date: how, when left entirely to his own resources, he fell into ridiculous traps: against all Greek precedent he provided his Scythians with archaeologically accurate Scythian cauldrons; he placed a Nike above the King in the hunting scene, forgetting that no Greek ever looked 'upon the slaying of a wild beast as a fact worthy to be crowned by Nike'; worst of all he gave to the wind-gods that hover above the pyre of Patroklos the form of children, though the wind-god as *putto* is a conception entirely foreign to antiquity. Lastly Professor Furtwängler repeats his assertion that the inscription on the tiara is clumsily adapted (in ängstlichen unsicheren Zügen) from the celebrated inscription in honour of Protogenes (*C.I.G.* 2058) the rich citizen who

⁴ August 1896. This first article was answered by M. Héron de Villefosse in *Cosmopolis* for September, and by Theodore Reinach in the *Gazette des Beaux Arts* for the same month.

¹ See on this point Ulrichs' *Griechische Statuen im Republikanischen Rom*, p. 19, an important little 'Programm' which has escaped Furtwängler's notice; also my note on Plin. xxxv. 26 in *The Elder Pliny's Chapters on the History of Art*, p. 197.

² Benndorf, Niemann, and Tocilescu, *das Monument von Adam Klissi*, Vienna, 1895.

³ Cf. Petersen in *Röm. Mith.* xi. 1896, p. 104 ff.

helped to replenish the city's empty coffers when Olbia was sorely pressed for costly gifts—*δῶρα*—by the barbarous King Saitaphernes. 'It is absurd to suppose that the wild, nomadic king who wanted gold, solid gold, was to be appeased by a Greek honorary inscription, by illustrations to Homer, and by little pictures on thin gold foil . . . But the forger with insufficient historical knowledge conceived the notion of fabricating one of the 'presents' mentioned in the Protogenes inscription, and of thus satisfying the popular craving for tangible witness to the truth of literary tradition. Many a 'relic' has ere now owed its existence to the same craving.'

A mere outsider may be allowed to feel surprise at the wealth of learning and of argument expended upon this ugly tiara as much by those who impugn as by those who champion its genuineness.

The reprint *Statuenkopien* should have a special interest for English archaeologists, as giving a detailed and richly illustrated description of a number of Greek statues at Ince Blundell Hall, Woburn Abbey, and Cambridge. Especially noteworthy are the superb Zeus and Theseus from Ince (plates I.–III.). A statue in the Villa Pamfili (pl. X.) is brought into connexion with the 'Mother of the Gods' of Agorakritos. The important introductory pages contain a first attempt towards distinguishing between various classes of copies. During the first or creative period of Greek art we only find 'studio copies,' school adaptations, derived or kindred conceptions, free imitations on coins, gems or vases. Copying proper begins in Pergamon, and coincides with the rise of the systematic study of art-history; yet accurate copying with help of the cast and of pointing was, so to speak, the invention of Pasiteles of Naples whose *quinque volumina nobilium operum in toto orbe* Furtwängler represents as a sort of descriptive catalogue of all the extant works best worth copying. The significant result of this first article is to show that we now know of a sufficient number of *signed* copies to enable us to bring some order among the unsigned ones also.

EUGÉNIE SELLERS.

Munich.

MEMPHIS AND MYCENAE.

Memphis and Mycenae; an examination of Egyptian Chronology and its application to the early History of Greece, by CECIL TORR, M.A. *Damnabitque oculos. Ovid.*

Cambridge University Press. 1896. 8vo. pp. xii. 74, and a folding table. 5s.

THE current statement 'that the Mycenaean age in Greece can definitely be fixed at 1500 B.C. or thereabouts, on the strength of evidence from Egyptian sources,' really consists of 'a pair of propositions; one being that the Mycenaean age in Greece was contemporary with the reigns of certain Kings of Dynasty XVIII. in Egypt; the other being that these kings were reigning there at some such date as 1500 B.C.' (p. i.) Mr. Torr denies both of these propositions; the first on the ground that the evidence is insufficient; the second on the ground that astronomical calculations are inapplicable to Egyptian chronology, and that the only safe reckoning is to construct from existing documents a chronology of minimum intervals from the Persian conquest of 525 B.C.

On the first of these, it is unfortunate that Mr. Torr has confined himself to a negative argument, and has reserved the whole of the positive conclusions which he claims to draw from purely Greek evidence (pp. i. and 65). And further, whatever the value of his statements may be on these, or upon the Egyptian chronology and kindred subjects which he discusses in the earlier chapters, his account of the archaeological evidence is frequently inaccurate and misleading in fact, and inconclusive in argument.

For example, in discussing the mass of rubbish at Tell-el-Amarna (p. 65) where Mykenaeen vases were found mixed with XVIIIth Dynasty rings and scarabs, Mr. Torr asserts that 'in order to maintain the notion that these Mykenaeen fragments are contemporary with those kings of Dynasty XVIII., one must suppose that when the people broke a vase of coarse Egyptian ware, they left the fragments lying about promiscuously; but when they broke a vase of delicate Mykenaeen ware, or even of Phoenician glass,¹ they carried the fragments out of the city and threw them away upon this piece of ground outside. And this does not seem likely.'

This is a misstatement of the case. This 'piece of ground outside'—some three furlongs, in fact, from the town—is a mass of rubbish some hundreds of feet in diameter, and, as Mr. Torr admits, averaging a foot in thickness. It consisted, as its discoverer expressly states, mainly of pot-

¹ Mr. Torr ignores Prof. Petrie's practically conclusive argument that this glass was made at Tell-el-Amarna.

tery,—some 20,000,000 sherds, by a rough estimate of its cubic content; and this enormous mass included no types characteristic of any Dynasty but the XVIIIth, and most of those which are peculiar to it.

Now it was 'scattered throughout the whole area' of this enormous mass of rubbish (Petrie, *Tell-el-Amarna* p. 15) that the 1329 pieces of Aegean pottery, and 'some dozens of objects with the names of the royal family' were found. The fact (p. 65) that pottery with XVIIIth Dynasty inscriptions was not found on this site is counterbalanced by the fact that these equally valid date-marks were so found. Mr. Torr as elsewhere insists on the negative, but refuses to admit the positive evidence.

Consequently, as Prof. Petrie says, 'Here we have not to consider isolated objects about which any such questions (of misplacement) can arise, nor a small deposit which might be casually disturbed, nor a locality which has ever been reoccupied: but we have to deal with thousands of tons of waste heaps, with pieces of hundreds of vases, and about a hundred absolutely dated objects with cartouches.'¹

He further considers it clear, from the dateable objects found in the rubbish heap, and quoted by Mr. Torr, 'that the mounds belong to a very little longer time than the reign of Akhenaten'² (Chu-en-Aten): and that there is no reason to suspect any admixture of later objects, either native or imported. In face of this definite statement, Mr. Torr insists that 'there was nothing whatever to indicate that the Mykenaeen and Phoenician fragments were thrown away there at the same date with the broken rings and scarabs,' (p. 65). But he produces no evidence either that the place was inhabited at all at any other date than the short period in the XVIIIth Dynasty which Prof. Petrie assigns to it: or that Prof. Petrie's method of dating by the style of the native pottery is unsound: or that there was any trace of subsequent additions to the rubbish-site, other than the Mykenaeen and 'Phoenician' fragments; he gives no explanation how the latter became distributed through the whole mass of rubbish: and in any case he fails to show that the Mykenaeen fragments, if they were not contemporary with the rubbish, were not already there before it was deposited.

¹ *Tell-el-Amarna*, p. 17. [It should, be observed that Mr. Torr has discussed Mr. Petrie's account in earlier numbers of this review (vol. vi. pp. 127 sq., and vol. viii. pp. 320 sq.).—G. E. M.]

² *Tell-el-Amarna*, p. 16.

Another misstatement of the same kind, but two-fold, follows in the next paragraph, where Mr. Torr makes the unsupported assertion that the foreign pottery found by Prof. Petrie in the rubbish heaps outside Kahun 'is mainly of the types that come to light at Naukratis and other places occupied by Greeks between 700 and 500' (p. 66) without a hint that this has been even questioned. In the first place, only four of the published fragments (*Illahun*, Pl. I. 4. 6. 10. 12.) could be mistaken by anybody for any known fabric of Naukratite pottery. In the second place, their discoverer, who was also one of the original excavators of Naukratis, distinctly states (*Illahun*, p. 10) that they are neither Naukratite nor of any later style known to him. This conclusion is based on differences alike of the clay, the glaze, the paint, the forms of the vases, and the scheme of ornament. In the third place, the very fragments which are least unlike Naukratite ware have been lately recognised, by identity alike of clay, glaze, paint, form, and ornament, as a local Cretan fabric.³ This Cretan pottery is found in undisturbed Cretan tombs which contain scarabs of Egyptian fabrics which are characteristic of the XIIth Dynasty and no other.⁴ Here, as in the case of the XVIIIth Dynasty scarabs of Mykenae and Ialykos, Mr. Torr ought to show why the ancient Cretan connoisseurs specialised in scarabs of the Twelfth Dynasty, and how they were enabled to reject late forgeries, and secure only specimens of the genuine fabric and materials; or if he refuses to accept these scarabs as of XIIth Dynasty style, he ought to give grounds for his opinion, instead of tacitly assuming that they are of some later date.

From this unsupported assertion that the Kahun pottery is Naukratite, Mr. Torr infers 'the futility of arguing that things must date from the same period, if they happen to be discovered in the same deposit.' (p. 66). If his premise is false, we must await further evidence before accepting his conclusion; yet it is this conclusion which underlies the whole of his argument in this chapter, and this is the only evidence which he brings to support it.

But let us take this conclusion, and apply it to the argument as stated by Mr. Torr.

³ Myres, *Proc. Soc. Antiq.* N.S. xv. (1895) 273: cf. Mariani, *Mon. Ant.* vi. (1896) Pl. viii. 5.

⁴ Evans, *Cretan Pictographs*, 1895, Appendix; cf. p. 57 = *J.H.S.* xiv. p. 327. I have seen the scarabs independently, and entirely agree with Mr. Evans' conclusion.

(1) If 'things which are discovered in the same deposit' are not necessarily of the same date, what becomes of Mr. Torr's argument from the contents of the same vault in the Apis sepulchres (p. 10), or from a collocation of mummies (p. 25)?

(2) If two sets of objects are not of the same age, one set must of course be older than the other; but it is a further question which is the older. Now Mr. Torr admits, rightly or wrongly, that the Rekh-ma-Ra tomb represents objects of 'Mykenaeen' workmanship already in the time of Thothmes III. (Men-cheper-Ra). It is therefore open to any one to argue, as against Mr. Torr at all events, that at Tell-el-Amarna the Mykenaeen potsherds are the prior ingredient in the rubbish heap, and not the scarabs of Thothmes III. and later kings; and in any case Mr. Torr's argument brings us no nearer to a decision whether scarabs of Dynasty XVIII. have been dropped on a Mykenaeen site, or Mykenaeen fragments on one of Dynasty XVIII.

(3) The same is the case with the deposit at Kahun, until Mr. Torr has established his identification of Naukratite pottery therein. The deposit must date 'at latest' from a period before the decline of the town¹; but Mr. Torr has still to show that the Aegean ingredient of it is not altogether earlier, for Prof. Petrie says that 'this Aegean pottery was found in *and under* these rubbish-heaps.'²

(4) Similarly Mr. Torr has still to show that the coffin of Pinetchem's grandson was not buried in an old tomb, and that part of the former equipment was not left lying there, or even used again for the new occupant. This is an occurrence which can be amply illustrated in Egypt, in Cyprus, and in fact, everywhere where chamber-burial was in vogue.

In discussing the XVIIIth Dynasty scarabs, &c., found at Mykenae and Ialysos, Mr. Torr displays no knowledge of any mode of dating Egyptian objects except by their inscriptions. He admits the criterion of style in a department of Mykenaeen archaeology where he can claim that it suits his theory (p. 69). Where it goes contrary, he ignores this class of evidence altogether. Thus he treats the scarab from Kamiros inscribed Chufu, as of the same value as those from

Ialysos inscribed Amenhotep III. and Thii, whereas the one is a XXVth Dynasty forgery of a common type,³ and the others are of regular XVIIIth Dynasty fabric, and of a series of which forged scarabs are apparently unknown. Mr. Torr seems to assume that a scarab is forged unless it can be demonstrated to be genuine. With our present knowledge of styles and fabrics the opposite assumption is at least equally tenable. Even Mr. Torr probably does not presume all Roman bronze coins to be forgeries of the age of Gallienus, in spite of the fact that such forgeries are recognised and common. And there is no more difficulty in detecting a XXVth Dynasty scarab, in spite of an early inscription, than in detecting those coins of Gallienus.

In any cases, however, in which the fabric is not decisive against a late date of manufacture, the evidence of a single scarab is of course very weak indeed. But when scarabs of several kings are found together, the probabilities, if the scarabs were mere ornaments or heirlooms, would be so greatly against the occurrence together of scarabs of consecutive or nearly consecutive reigns, that when these do occur together, they may be regarded as very probably fixing the date of the group in the place where it occurs.⁴

Now the evidence of the scarabs on Mykenaeen sites is very much strengthened by the fact that both at Mykenae and at Ialysos all the imported porcelain objects of recognisable fabrics are of XVIIIth or XIXth Dynasty styles.⁵ The probability is thus proportionately strengthened that they were all imported within the period to which they belong in Egypt. Before Mr. Torr can secure his own position, he will have to bring evidence not merely that they are *not* of XVIIIth Dynasty fabric, but that they *are* of some recognised fabric which better suits his theory.

The hypothesis of heirlooms, like Mr. Torr's rejection of the argument from grouping, cuts both ways. Which is the more probable heirloom, a rare foreign vase, or a perishable article of everyday use like a wooden kohl-tube (p. 63-4) even if the latter bears a royal cartouche? The latter, by the way, shows no sign of long use: and royal cartouches were too common on household articles to confer any special value.

¹ *Illahun*, p. 9. 'From their position no later people would have accumulated these heaps...The external rubbish-heaps must belong to a time when the town was full. And their contents agree to that early date.'

² [For Mr. Torr's comments see *Class. Rev.* vi. p. 130.]

³ Cf. Men-ka-Ra in a Ptolemaic or Roman tomb at Amathus (Brit. Mus. 172) and the ubiquitous 'Naukratite' forgeries of Men-kheper-Ra (Thothmes III.).

⁴ E.g. Neb-mat-Ra and Neb-kheferu-Ra at Gurob (Torr, p. 63), and the numerous Eighteenth Dynasty scarabs and rings at Tell-el-Amarna (Torr pp. 64-65).

⁵ *J.H.S.* xii. p. 273 ff.

He also thinks¹ that the occurrence of XVIIIth Dynasty scarabs at Ialysos, and the popularity of 'Memnon' in later Greece, are explained by the foreign origin of Queen Thii. But, in syllogistic form, 'some foreigners are not Greeks.' Queen Thii came from N. Syria, perhaps even from beyond the Euphrates. The popularity of Maria Theresa dollars in Abyssinia is not explained by a marriage alliance between Austria and Spain.

In a short Appendix Mr. Torr reprints from the *Academy*, for the benefit of Mr. H. S. Washington (p. x), a refutation of M. Fouqué's theory that the eruption of Thera, which buried a prehistoric settlement, might be placed as early as, or earlier than, 2000 B.C. Volcanoes are capricious creatures, and if there is method in their madness, no one has yet detected it. But after correcting M. Fouqué's history, Mr. Torr himself falls into a geological error.

(1) Mr. Washington, with whose conclusions Mr. Torr says that he agrees, shows clearly on geological grounds that the whole of the pumice had been laid down and consolidated before the present cliff-face was formed (v. Washington, *Am. Journ. Arch.* ix. p. 512). In many places the deposit is deeply eroded, and covered with rolled gravel; and none of the recorded eruptions have been severe enough, or near enough to the cliff-face, to cut it back appreciably. Moreover the story of droughts in Hdt. iv. 147 accords with the present state of the island, thickly covered as it is by this series of pumice-beds, which absorb all surface water.

(2) If the mediaeval eruptions covered the island thickly with pumice, this ought to be represented above the rolled gravel and shingle-beds which overlie the older pumice-beds. But neither Mr. Torr, nor Mr. Washington, nor M. Fouqué notice this vital point, even in describing the viii century buildings; and as a matter of fact such mediaeval pumice-beds cannot be identified. Theophrastus probably exaggerated the eruption of 726 A.D., and Mr. Torr certainly exaggerates the amount of pumice which has fallen in Thera since the viii century B.C. When he claims a large part of the existing pumice as mediaeval, it is a fair question, which and what thickness of the existing beds does he mean, and on what geological evidence does he rely?

(3) In any case, the viii century buildings are above the consolidated pumice, and

the prehistoric settlement is below all the pumice there is, for it stands directly on the lava. Now there is no evidence of an eruption between the Hellenic colonisation of Thera and 196 B.C., and on Mr. Torr's principles we must not assume one. Therefore the great eruption, or eruptions (for soil was formed and grass grew in an interval between the pumice-showers), probably took place before the Hellenic settlement, and certainly before the foundation of the viii century buildings. All this agrees with the Hellenic tradition (a) that the island was called Στρογγυλή and Καλλίστη when 'colonised by Kadmos,' whereas in its present state it is neither 'round' like its namesake Stromboli, nor 'very beautiful'; (b) that, at a date roughly reckoned in generations to the middle of the second millennium, this colony perished utterly, and the island lay desolate; (c) that the Hellenic colonisation came later, and that the island was then called Θήρα, which suits its present condition admirably. The clear inference from all this is that the great eruption was traditionally known to have preceded the Hellenic settlement, i.e. took place by genealogical reckoning in the ix or x century 'at latest,' since which time the island has altered very little.

We turn now to Mr. Torr's revision of the Egyptian dates, and note in the first place that the two parts of his argument hang closely together. He wants minimum dates in Egyptian chronology, because he wants to reduce the interval between the Mykenaeon and the Hellenic civilisations. As long as intermediate stages were unknown between the best Mykenaeon and the earliest Hellenic art, this *horror vacui* was not without excuse. But, fortunately, recent discoveries in the Aegean, in Crete, and in Cyprus, have indicated clearly a long series of intermediate stages of civilisation, and the problem now is rather how to find room for the whole series within the chronological limits, than how to draw together the two edges of an apparent gap.

And it is here that Mr. Torr's results are of positive value. 'A statement is current,' as he would say,² that the golden cups from Vaphio represent the goldsmith's art of the vii century. Mr. Torr's argument shows that Pinetchem, in whose grandson's tomb a solitary and belated Mykenaeon vase was found, 'came to the throne in 876 at latest.' He brings no good evidence to show that, if it does not belong to the tomb, it is not earlier; and all the other examples which

¹ Unless (p. 69) 'this region' and 'that region' in the same sentence refer to the same country; which would be very queer English.

² *Times*, Jan. 6, 1896; *Academy*, Jan. 11, 1896.

he quotes are very much earlier. So we may hope to hear no more of that theory at all events. In any case, the discovery of a very *late*-Mykenaeen style, in Cyprus for example, proves no more (but also no less) about dates at Mykenae or Ialysos than the discovery of very *early* objects at Kahun.

On the other hand, though he refuses to date any extant Mykenaeen object at all so early, he admits that Mykenaeen objects are represented in the tomb of Rekh-ma-Ra, in the time of Men-khefer-Ra (Thothmes III.) (p. 67); in which case, it is difficult to see what is gained by disputing the date of this or that vase, when all the extant specimens are of later dates than Thothmes III.

The current chronology of Egypt is based on the assumption that the Egyptians used a calendar year of 365 days and no leap year: so that the natural year of approximately $365\frac{1}{4}$ days completed a cycle of retardation in 1461 calendar years, carrying with it the natural seasons, the rising of the Nile, and the heliacal risings of the stars; together with all feasts which were regulated thereby. From this it follows that if the calendar dates of the same phenomenon or feast are known for two different calendar years, the interval between those years can be directly calculated from the discrepancy.

Mr. Torr argues (1) that in any case the real duration of the 'Sothic cycle' ending 139 A.D. (as used by Censorinus) would not have been 1461 years but 1457; (2) that it would have begun and ended on different dates in different parts of Egypt; (3) that it was invented by Hellenistic astronomers at Alexandria (p. 57); (4) that it is not presupposed or recognized by certain Rameside calendars which he quotes (p. 59).

With regard to points (1) and (2) Mr. Torr may set his mind at rest; for if he will consult any of the principal contributions to Egyptian chronology from Biot downwards—of whom he quotes not one throughout the chapter—he will see that these elementary astronomical facts have not been ignored in the calculation of the current chronology. The fact that Alexandrian chronologists used imperfect data does not affect the validity of the method, or the general coherence of their results. *E.g.* Theon of Alexandria puts an 'era of Menophres' in 1322 B.C. Menophres, of whom Mr. Torr knows nothing (p. 65), may well be Men-peh-Ra (Ramesses I.) whose reign is dated 1328–1326 by downward reckoning from Mahler's date for Thothmes III. (cf. Petrie, *Hist. Eg.* II. 33).

Meanwhile, Mr. Torr says (p. 57) that

some Egyptian calendars were calculated for a year of 360 days (*e.g.* Papyrus Ebers); and others for an astronomical year of $365\frac{1}{4}$ days; but he ignores a large number of facts which show that ordinary Egyptian reckoning recognised a cycle of some sort, and give consistent results only when combined on the hypothesis that this cycle was that of 1461 years, afterwards calculated by Alexandrian astronomers. For example Herodotus (II. 4) contrasts the ordinary Greek year of 360 days, by which the natural seasons shifted appreciably from year to year and were redressed by intercalation, with the Egyptian year current in his time, where five days, intercalated annually, kept the seasons redressed from year to year. This passage proves the use of a calendar year of 365 days in the fifth century. That is all that is required to warrant the application of the Sothic reckoning to Egyptian chronology. Mr. Torr may be right or wrong in saying that the cycle of 1461 years was not calculated or applied to historical purposes till the Ptolemaic age: but that does not affect the question whether either Censorinus or Mahler is justified in reckoning dates by the aid of it.

But the use of a year of 365 days in Egypt can be traced much further back than the fifth century. A series of XVIIIth Dynasty documents shows that the date of the Sothic festival was systematically altered by seven days every thirty years and that this change was celebrated by a greater feast, the *Sed*-festival. In a series of twelve consecutive *Sed*-festivals, only three are unrepresented by extant inscriptions, and one of these falls in the 'heretic' reign of Akhenaten: and of the remainder five expressly note the month and day of the festival. Now these regularly recurring dates will not work out on any hypothesis but that of a year of 365 days; and as the *Sed*-festivals recur in inscriptions of other reigns at considerable intervals, the presumption is that the year of 365 days was normal. It is true that Ramessu II. started a new series of *Sed*-festivals every third year from his thirtieth onwards; but that he did not interfere with the astronomical *Sed*-festival is shown by the El Kab inscription of his forty-first year.

And yet again, an inscription of the IVth Dynasty gives a calendar of twelve months of thirty days, with five intercalary days at the end of the year, which is exactly the system described by Herodotus. This disposes of the account of the five days given

in the Book of the Sothis (*Synce* p. 123), and justifies the calculation of dates by astronomical methods under the Old Kingdom: where an inscription, which dates the Nile-flood, and corresponds to 3350 B.C., gives a date of 3410 B.C. for the beginning of Dynasty VI., as against 3503 by dead-reckoning from the lists.¹

Mr. Torr's alternative chronology is constructed from a number of official or semi-official documents, which give a continuous genealogy upwards from the accession of Psammetichos in 664 B.C. to the third year of Rameses Heq-mat-Ra (p. 34). This genealogy, if the generations, fifteen in number, were given the Greek conventional length of thirty years, would give 1117 B.C. for the accession of Heq-mat-Ra which is not far from that given by astronomical reckoning. But Mr. Torr goes further than this. His object is to produce a chronology of minimum intervals, and he succeeds in reducing the accession of Heq-mat-Ra from 1117 B.C. to 942 B.C. 'at latest' by the following ingenious methods.

(1) No king is reckoned to have reigned longer than the last year of which a dated document is known to Mr. Torr. This is as though he were to revise Ptolemaic chronology by cutting down the reigns to the year recorded on the latest known coin in each case.

(2) If a king seems to have reigned unreasonably long, he may be assumed to have reigned *de jure* and not *de facto*, like Charles II. who reckoned from 1649, though not 'recognised at Westminster' till 1660. Thus Mr. Torr proposes to annihilate the twenty-three years of User-mat-Ra Takelot (p. vii.) with the conjecture that he reigned *de facto* for a few months, and told lies about the rest; on the ground that 'No king of Egypt would have reigned for all those years without making himself conspicuous upon the monuments.' Let us hope that Mr. Torr's exertions may save him at all events from that condemnation.

(3) If generations mount up provokingly fast, three or four successive occupants of a hereditary office may be assumed to have been brothers (p. 9): in spite of the fact that they all bear the title of 'Royal Son.'

(4) Similarity of name is good evidence of identity of person: e.g. (p. 24) Anupuat, royal son of Rameses, is identified on weak evidence with Aput, son of Hetch-kheper-Ra Sheshenk: (p. 13) two Nemarts and (p. 14) two Usarkens are identified. Edward II. and Edward III. are not identi-

cal, though each had a father Edward, and each held the title of Prince of Wales.

Moreover, if Skemiophris (p. 48) can represent Sebek-em-sas; (p. 29) Psusennes, Paseb-chanu; and Sivi, Sabako; it is a little hypercritical to refuse Aquaiusha for 'Αχαιφοί, as Mr. Torr does, ignoring the fact that this is only one of a long list of equally close transliterations, and that the cogency of such a list is cumulative.

(5) Personal names go in alternate generations in many Egyptian families; but in a work which professes to take nothing for granted, the frequent use made of this canon to piece fragmentary genealogies together needs explanation.

It is a corollary from this and the last-named proposition, that a man is his own grandfather unless there is documentary evidence to the contrary: a genealogical canon which we recommend to Mr. Torr's serious consideration. This might be applied to reduce even his minimum by one-half.

(6) The unknown name of a brother may be recovered from the masculine form of the name of a woman whom it is convenient that he should have had as sister and as wife (p. 7). This also needs justification.

(7) The Apis was not an occasional prodigy, but the succession of Apis bulls was continuous, so that the death of one Apis necessarily coincided with the birth of the next. In which case, we should expect an explanation (1) how the new Apis was brought to birth so conveniently, (2) why its birth was ever chronicled at all, if the date was fixed by the death of its predecessor, e.g. (p. 10) the Apis dating of Sheshonk and Bocchoris.

(8) If no Apis died in a king's reign, he was not 'recognised at Memphis'—such is fame! Consequently he must have reigned somewhere else, and someone else, in whose reign an Apis died, must have been king at Memphis meanwhile; consequently all kings who failed to survive an Apis form 'parallel dynasties' with those who succeeded in doing so.

If the negative evidence is quite complete for whole Dynasties, a probability is established, but no more: in any case the argument is negative; and is there any evidence that no Apis bulls are buried elsewhere?

The net result of this minimum chronology is to reduce the interval from Psammetichos (XXVI. 1) to Ahmes (XVIII. 1) from 923 years to 607, and the accession of the latter from 1587 B.C. to 1271 'at latest.'

¹ Petrie, *Hist. Eg.* i. 253.

Above this point Dynasties XIII.—XVII. are extinguished utterly—because, presumably, they did not ‘make themselves conspicuous’ to Mr. Torr;—for Sequenen-Ra and Apepi, who did, are admitted on sufferance—so that Amenemhat (Mat-cheru-Ra) of Dynasty XII. is placed in the generation immediately above Ahmes of Dynasty XVIII.

Unfortunately the genealogy, which is the valuable part of the essay, is not carried continuously beyond 939-40¹ ‘at latest’; but it is in this section that the main reductions which affect the Mykenaeen question are made. It is perhaps worth pointing out that the fifteen generations in question are compressed into 275 years. It is true that six of them are in the female line, but the average length of a generation, from birth to birth—eighteen years and four months—is surely a ‘minimum interval.’ The Jewish kings have an average of exactly twenty years, which is very much higher. Moreover, if Mr. Torr’s assumption, that family names went in alternate generations, is sufficiently well founded for *his* purposes, it proves also that fully half of the children in this list were not eldest sons; which of course lowers the birth-to-birth average of parental ages. Now as an average presumes that some are over, and some are under the average, the physical limit is very nearly reached in the latter cases.

So much for the theoretical aspect of Mr. Torr’s chronology. It has this commonplace practical difficulty which will much delay its adoption, that every new discovery will shift the whole series above it: for there are no fixed points except at the bottom. However, we now know the worst: any change henceforward must be in an upward direction, and we wish Mr. Torr, as discoveries proceed, a complete and a rapid recantation.

JOHN L. MYRES.

¹ The third year of Heq-mat-Ra, p. 34.

MONTHLY RECORD.

ITALY.

Este.—An interesting discovery has been made here in the shape of a well or shaft for the drainage of a house. The mouth of it was closed by a slab of stone, above which were regular layers of earth, slabs of mortar, small stones, and more mortar. It was used for collecting water in rainy seasons, so as to keep the moisture from other parts of the building. The shaft is built of eight courses of stone, and was found full of slabs of stone inside, together with tiles, all artificially arranged so as to allow the water to

run through. Two similar arrangements have hitherto been found in Italy.¹

Bologna.—Some inscriptions found during recent excavations have now been published; none are of very much interest except a *cippus* of L. Statorius Bathyllus, with a head of Medusa in the tympanon and a rosette on either side. Below the inscription are a pair of compasses and a plumb line, indicating that Bathyllus was an architect. [For similar subjects, see Blümner, *Technologie*, ii. p. 236, and Durm, *Baukunst*, p. 361.] Another *cippus* of Q. Valerius Restitutus has a relief representing an *aurifex brattiararius* (cf. Jahn in *Ber. d. Sächs. Gesellsch.* 1861, pl. 7, Fig. 2=Blümner, *op. cit.* p. 312).¹

Arezzo.—Five tombs covered with tiles, containing fragmentary vases, have been found in the bed of the river, showing that its course must have been originally different. At a distance of one mile from the city Etruscan remains have been found, consisting of a tomb with cinerary urn, an inscription, and fragments of Campano-Etruscan ware of the second century B.C. The urn is inscribed *Velia Vetui*; the tomb is covered with a slab of sandstone on which is inscribed *V. Caini C. Rucu Ceinal*. Another urn was found with the inscription *Larhi Ti Aneina*. All these are the names of various Aretine families.¹

Corneto-Tarquintii.—A find has been made of archaic Greek vases and others of local fabric; also bronze fibulae and other remains. Among the vases was an aryballos in the form of a helmeted head, well executed, in the Rhodian style; the helmet has a hook in front, probably the *φάλαξ*, as is seen on the Clazomenae sarcophagi.¹

Rome.—An interesting *cippus* has come to light on the Via Latina, with an acrostic inscription which runs as follows:

Moribus hic simplex situs est Titus Aelius Faustus,
Annis in lucem duo de triginta moratus,
Cui dederant pinguem populis praebere liquorem
Antoninus item Commodus simul induperantes.
Rara viro vita et species rarissima; fama
Invida, sed rapuit semper fortuna probatos.
Ut signum invenias quod erat dum vita maneret
Selige literulas primas e versibus octo.

This Macarius was the son of a freedman of Antoninus Pius, and from A.D. 176-180 superintended the public distribution of *mustum* or of oil (see line 3).¹

Sala Consilina, Lucania.—Some archaic tombs have been investigated, containing fragments of Corinthian ware, and some black-figured vases; a hydria of the common archaic Italian type, and other bronze vessels, the finest of which is an oinochoe, the handle of which is formed by the figure of a nude man leaning back, a common Etruscan type.¹

Carife, Apulia.—Two vases have been found here, containing a treasure of 13 silver and 103 bronze coins, 17 of the latter being cast, the rest stamped. The cast coins are all Roman fractions of the *as*; among the others are coins of Neapolis, Arpi, Heraclea, Thurii, and Aquilonia.¹

Reggio.—A bath has been discovered, of considerable size, with frigidarium, hypocaust, mosaic pavements, and curved marble seats (*scholae*); also a conduit of terracotta, beneath a mosaic staircase. A marble slab was found representing a gabled edifice, with a jug and patera in the tympanon, and remains of an inscription ΠΡΥΤΑΝΙΚ·ΚΑΙ | ΑΡ·
ΧΩΝ·ΕΚΤΩΝ | ΙΔΙ]ΩΝ . . . | . .

¹ *Notizie dei Lincei*, April-June 1896.

ΥΙΟC·PH | . . ΥΤΑ. Kaibel (nos. 617, 618) gives similar inscriptions found here.¹

Terranova (Gela).—An interesting archaic Greek inscription has been found here, the oldest as yet known from Sicily. It is *Βουστροφρόνδον*, and runs: ΠΑΣΙΑΔΑΦΟΤΟ | ΕΞΕΤΑΡΚΑΜΑΣ | ΠΟΙΕ Πασιαδάδου τὸ σῆμα ἡ Κράτης ἐποίησεν. By comparison with the Geloan inscription at Olympia (Roehl, *I.G.A.* 512a) we may date this not later than the end of the sixth century B.C.¹

GREECE.

Patras.—In the quarter of the town known as Psila Alonia a very fine mosaic of Roman date has come to light. The portion at present above ground is about 10 × 20 ft., and is enclosed by a border, but it appears to extend still further on one side. On the part already cleared are two subjects: (1) a group of nude athletes after the conclusion of the games; some wear laurel-wreaths and hold branches of wild olive; others hold shields or the diskos; and one is scraping himself with a strigil. (2) A figure dressed in purple accompanies a group of dancers on the lute; women in short dresses play the flute and stringed instruments. The colours are very vivid, but the part containing the musical scenes is not well preserved.²

Thessaly.—Two bee-hive tombs have come to light on the south slopes of Mt. Ossa, built of stone, with *δόμοι*. Very little was found in them except pottery, which is rather of a prehistoric than Mycenaean character, the shapes recalling the fabrics of the Cyclades, but the decoration is later, chiefly geometrical painted patterns.

At Karditza a very interesting archaic inscription has been found, on a bronze tablet. It is eleven lines in length, and it is to the effect that on the motion of Eaylos the Sthetonii gave Sotairos of Corinth and his family protection and indemnity and proclaimed him a benefactor, for rescuing the silver and gold of Orestes, son of Pherocrates, which was nearly lost on the way to Delphi.³

Thera.—Herr Hiller von Gaertringen has brought his excavations to an end. He has identified the city on the slopes of Mesa Vouno with the ancient Thera, while Oea has been recognised in the remains on the sea-shore near the modern town. An ancient

necropolis has also been explored between Mesa Vouno and Agios Elias, containing tombs of the archaic period, very rich in pottery and terracotta statuettes. Some vases of the Thera type were found, and are among the best specimens of the kind; others are of Peloponnesian, Boeotian, and Cretan character [query: Mycenaean?], showing the high development of Aegean trade at that date. The number of inscriptions found in Thera has now been brought up to 650.⁴

ASIA MINOR.

Valley of Upper Euphrates.—In 1894 this region was explored by Messrs. Hogarth and Yorke, with the object of discovering traces of the system of defences organised by the Romans on the eastern frontier of the Empire. They travelled from Mersina by Samsat (Samosata) and Erzinjian to Trebizond. The road from Mersina to Samsat is fairly well known, but one or two new inscriptions turned up, one at Missis (Mopsuestia) being a milestone of Valentinian, Valens, and Gratian; four more were found at Samsat. The rest of the way to Erzinjian was quite unknown, and important geographical results have been obtained. Between Erzinjian and Trebizond the identification of Sadagh with Satala has been finally settled by the discovery of inscriptions at that place relating to the fifteenth legion (Apollinaris), which was known to have been quartered at Satala.

As regards the Roman roads and defences the chief results are as follows: the Peutinger route from Melitene (the centre of the system in this district) has been shown to go a different way from the Antonine Itinerary, over the existing Roman bridge at Kiakhta; but what line it took over the Taurus is impossible to ascertain. On the road from Melitene to Satala the position of Dascusa and Dagusa has been distinguished and fixed with some probability, and other small points have been cleared up. Hardly any milestones exist in this region, and between Samosata and Satala there are only five with names of Emperors. The remains of defensive works are also very slight, probably owing to the fact that they were not much needed. In the first and second centuries of the Empire, Armenia was practically a Roman province, and consequently the frontier did not require to be protected against it.⁵

H. B. WALTERS.

¹ *Notizer dei Linciei*, April–June 1896.

² *Athenaeum*, Oct. 10.

³ *Mittheil. d. deutsch. Arch. Inst.* 1896, pt. 2.

⁴ *Athenaeum*, Nov. 7.

⁵ *Geographical Journal*, Oct.–Nov. 1896.

SUMMARIES OF PERIODICALS.

Revue de Philologie. Vol. xx. Part 3. July, 1896.

Néron et les Rhodiens, P. Fabia. On the date of Nero's speech for the Rhodians we must follow Tacitus (*Ann.* xii. 58) who makes it 53 A.D., and not Suetonius (*Nero* 7) who puts it in 51. An inscription recently discovered and published by M. Hiller de Gärtringen informs us that in the first year of Nero's reign a Rhodian embassy came to him in consequence of a letter received by them, the contents of which we do not know. *Quelques passages de Phèdre*, L. Havet. Reads in iv. 20 [iv. 18. 3] *sinuque se ipse fovit contra misericors*: defends *astuens* of codd. in v. 1, 10 [12]; in v. 5, thinks a line has

dropped out between ll. 18 and 19. *Fragments de l'Épître prior des Clémentines recueillies sur les feuilles de garde d'un Parisinus: principales variantes*, C. E. Ruelle. *Sur les vers 602–627 du 6e livre de l'Énéide*, A. Cartault. Against the proposal of L. Havet to place ll. 616–620 after 601. *Plaut. Trin.* 540, L. Havet. Suggests *sacerrume* for *acerrume* of codd. *Corrections proposées dans Aristide Quintilien, sur la Musique*, C. E. Ruelle.

American Journal of Philology. Vol. xvii. 2. Whole No. 66, July, 1896.

On the Western Text of the Acts as Evidenced by Chrysostom, F. C. Conybeare. This text is best

given, though not in its entirety, in the Codex Bezae. It is here maintained that there once existed a Greek text of the type called Western, which was more comprehensive and older than the Bezan, and that this now lost text was the basis of an early commentary to which, in some form or other of it, both Chrysostom and Ephrem had access, so as to use it in their respective commentaries on the Acts. *Establishment and Extension of the Law of Thurneysen and Havet*, ii. L. Horton-Smith. Summarizes his results thus: In the course of the third cent. B.C. among the upper classes (but not before the beginning of the second cent. B.C. among the lower classes), in consequence of very open pronunciation of δ before η , (1) Prim. Lat. $\delta\eta$ became $\delta\eta$; (2) Prim. Lat. $\delta\eta$ became $\delta\eta$; and (3) the Prim. Lat. diphthong $\alpha\eta$ became the diphthong $\alpha\eta$ on its way to the later $\alpha\eta$. *The Classical Element in Brownings Poetry*, W. C. Lawton. A Physiological Criticism of the Liquid and Nasal Sonant Theory, H. Schmidt-Wartenberg. Concludes as follows: The reduction of a syllable consisting of an explosive + short vowel + nasal results in a decrease of the vowel quantity by one-half of its original value approximately. If the vowel is suppressed the initial consonant is lost also. The liquids, especially l , are more difficult to investigate; as their development in reduced syllables, however, corresponds to that of the nasals, this fact alone is sufficient to invalidate the liquid sonant theory also. The only book on Classical Philology which is noticed is Van Bleef's *Index Antiphonens* by W. H. Kirk.

Neue Jahrbücher für Philologie und Paedagogik. Vol. 153. Part 8. 1896.

Die dreiseitige basis der Messenier und Naupaktier zu Delphi, H. Pomtow. This is the parallel monument to the Olympic Messenian Naupactian memorial. The writer gives a description of the five larger blocks and the dedicatory inser., and then, comparing it with the Olympic memorial, attempts a reconstruction. *Zu Ciceros briefen an Atticus*, L. Polster. In v. 4, 4, reads *dumtaxat* for *dum acta et* [see Cl. Rev. ix. 429]. *Die älteste münze Athens*, G. Gilbert. Before Solon's time Attica had a coinage of the Aeginetan standard. Solon introduced the Euboic, and made a two-drachma-piece the chief Attic coin. Hippias replaced this by a four-drachma-piece. *Zu den namen der Kureten*, O. Höfer. As the names of two of the Carian Curetes, $\Delta\delta\sigma\pi\alpha\rho\delta\sigma$ and $\Pi\alpha\nu\mu\omega\rho\sigma$, are connected with titles of Zeus, so the third, $\Pi\delta\lambda\alpha\sigma$ or $\Sigma\pi\delta\lambda\alpha\sigma$, has lately been connected with an inser. found at Mastaura to Zeus $\Sigma\pi\delta\lambda\omega\sigma$. *Zu Andokides mysterienrede*, F. Schöll. Remarks on the text. *Zum delphischen Labyadenstein*, H. Pomtow. From the form of the letters we can with great probability assign the archonship of $\kappa\alpha\rho\pi\omega\varsigma$ herein named to the first decade of the 4th cent. B.C. *Zu Tacitus*, L. Polster. In Ann. i. 64 suggests *inter uida* for *inter undas* [see Cl. Rev. ix. 429]. *Zu biographie des Lucretius*, R. Fritzsche.

Chiefly on *Giris*' excellent book 'il suicidio di Lucrezio' (Palermo, 1895). We have not enough materials to come to a definite conclusion [see Cl. Rev. ix. 188, 240]. *Zu Tacitus Agricola*, W. Pfitzner. Maintains that in c. 24 an expedition to Ireland is referred to [see Cl. Rev. ix. 310]. *Zu Ciceros reden*, K. Busche. Critical remarks on several speeches. *Zur geschichte des feldzugs Hannibals gegen Scipio* (202 vor ch.). K. Lehmann. Supports his previous contention that the great battle between Hannibal and Scipio was fought not near Zama but near Naraggara in Numidia.

Rheinisches Museum. Vol. 51. Part 4. 1896.

Zur Handschriftenkunde und Geschichte der Philologie iv. R. Foerster. Upon the commentary of Cyriacus of Ancona to Strabo. *De Propertii poetae testamento*, Th. Brit. A detailed commentary on Prop. ii. 13. *De Francorum Gallorumque origine Trojana*, Th. Brit. Defends the MS. in Prop. ii. 13, 48. *Gallicus Iliacis miles in aggeribus* [see Cl. Rev. ix. 443], comp. Qu. Sm. vii. 611. *Neu aufgefundenen graeco-syrische Philosophensprüche über die Seele*, V. Ryssel. The same MS. from the convent on Mt. Sinai from which comes the treatise 'on the soul' [see Cl. Rev. sup. p. 77], contains also a series of 'sayings of philosophers' which belong to that collection of sentences which we already know from Sachau's *Inedita Syriaca*. German translations of these sayings from both collections are here given. *Excursus zu Virgil*, O. Crusius. (1) Origin and composition of the 8th Eclogue. (2) On the 4th Eclogue, especially on ll. 60-63 [see Cl. Rev. vii. 199]. Upon the much-vexed question of the *puer* Gibbon is quoted as saying (ch. xx.). 'The different claims of an older and younger son of Pollio, of Julia, of Drusus, of Marcellus are found to be impossible with chronology, history and the good sense of Virgil.' Gibbon says 'incompatible,' but no doubt he would be pleased to have his English corrected in a German periodical. *Delphische Beilagen*, H. Pomtow. (1) The years of the tyranny of Peisistratos in connexion with $\Delta\delta\eta\nu\alpha\lambda\omega\nu$ $\rho\alpha\delta\iota\tau\epsilon\lambda\alpha$. (2) The date of Pind. Pyth. vii. *Textkritisches zu Ciceros Briefen*, J. Ziehen. *Ueber den Cynegeticus des Xenophon I.*, L. Radermacher. Discusses the authorship [see Cl. Rev. sup. p. 313].

MISCELLAN. *Zu Ariston von Chios*, H. Weber. *Zur Epigraphik von Thyateira*, E. Ziebarth. A criticism on M. Clercs' *De rebus Thyatirenorum commentatio epigraphica* (Paris, 1893). *Die Heptanomis seit Hadrian*, W. Schwarz. All inscriptions in which mention is made of seven Nomes and of the Arsinoite are later than the foundation of Antinopolis, i.e. later than Hadrian. *Zu Statius Silven*, A. Riese. In iv. 3, 19, suggests *clavum* for *calvum* [see Cl. Rev. sup. p. 223]. *Zu Augustins Confessiones*, M. Ihm. In viii. 2, 3 reads *inspirabat populo Osirim*. *De inscriptionibus quibusdam christianis*, F. B. On the inscriptions found by P. Orsi in the catacombs at Syracuse, of dates 383-452.

MR. AGAR'S REVIEW OF THE OXFORD HOMER.

MR. AGAR, in the interesting review published in the last number, makes a series of criticisms and suggestions on the Homeric Hymns, and in doing so connects my name with a good deal that is not properly due to me. Let me call his attention to the words of the Preface: 'Hymnos Homericos post novam recensioem Alfredi Goodwin denuo

correxerit T. W. Allen, necnon breves notulas subiecit.' I have of course a general editorial responsibility for the book, but the text of the Hymns and the critical notes which accompany it are the work of Mr. Allen.

D. B. MONRO.

Oriel College, Oxford, Nov. 18.

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¹ The Index is by W. F. R. SHILLETO, M.A., formerly Foundation Scholar of Christ's College, Cambridge.
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